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729.

(Vol. XXXII.—September, 1894.)

REMOVAL OF ROCK 40 FT. BELOW SURFACE OF WATER, NORTH RIVER, N. Y.

By JOHN A. BENSEL, M. Am. Soc. C. E.

READ AT THE ANNUAL CONVENTION, JUNE, 1894.

WITH DISCUSSION.

The work, of which this is a description, consisted in the removal of about 2 000 cu. yds. of rock in the vicinity of Pier No. 14 (new), N. R., by the Department of Docks, City of New York.

The work was necessitated by an alteration in the use to which the pier was to be put. The pier having originally been built and completed, with the dimensions of 720 ft. in length and 75 ft. in width, and as for about the outer one-third of the length of this pier the bed rock rose to a grade at its highest of about 27 ft. below mean low water, and there being little or no mud overlying this rock, the construction necessitated the placing of rip-rap on this portion of the pier site.

The rip-rap was placed to a grade of 15 ft. below mean low water, except at the sides of the pier, where it was sloped to a grade of 20 ft. below mean low water. The location of the reefs removed is shown on Fig. 1.

This pier having been intended to be used for railroad purposes, the mud had been dredged to the usual depth of 25 ft. below mean low water in the slips north and south of the pier. After the pier had been completed, however, it was leased to the new American Line Steamship Company, in whose fleet are the steamships *Paris* and *New York*, and these ships drawing a maximum of 29 ft., it became necessary to make a greater depth of water along each side of the pier, the pier being also widened by the addition of 50 ft. along the entire length on the south side.

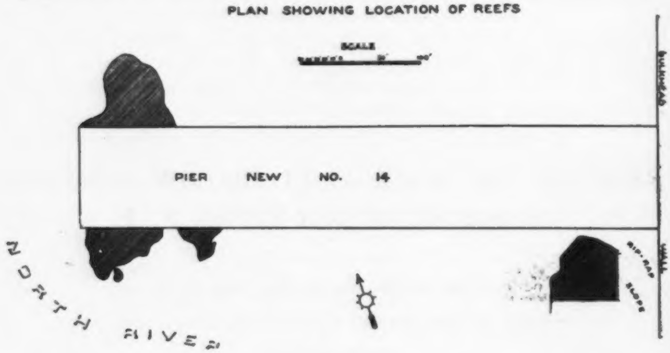

FIG. 1.

The American Line officials requested that the depth be made 35 ft. below mean low water.

The relation between the midship section of the steamships *Paris* and *New York*, with their greatest draught and the lowest recorded tide, is shown in Plate XXXV.

As the mean rise and fall of the tide at the location in question is about 5 ft., the operation of making a grade of 35 ft. below mean low water necessitated the use of appliances that would operate and do this work in 40 ft. of water, and also accommodate themselves to the rise and fall of the tide while in operation.

Operations were commenced under contract on July 25th, 1892, and were completed by the Department of Docks on September 30th, 1893. During this entire period the plant used by the Department was found efficient during all kinds of weather, even in spite of the North River ice, which was more than usually heavy during the winter of 1892–93.

DIAGRAM SHOWING RELATION
OF
MIDSHIP SECTION OF S.S.PARIS
TO
SLOPE OF ROCK BOTTOM AFTER EXCAVATION

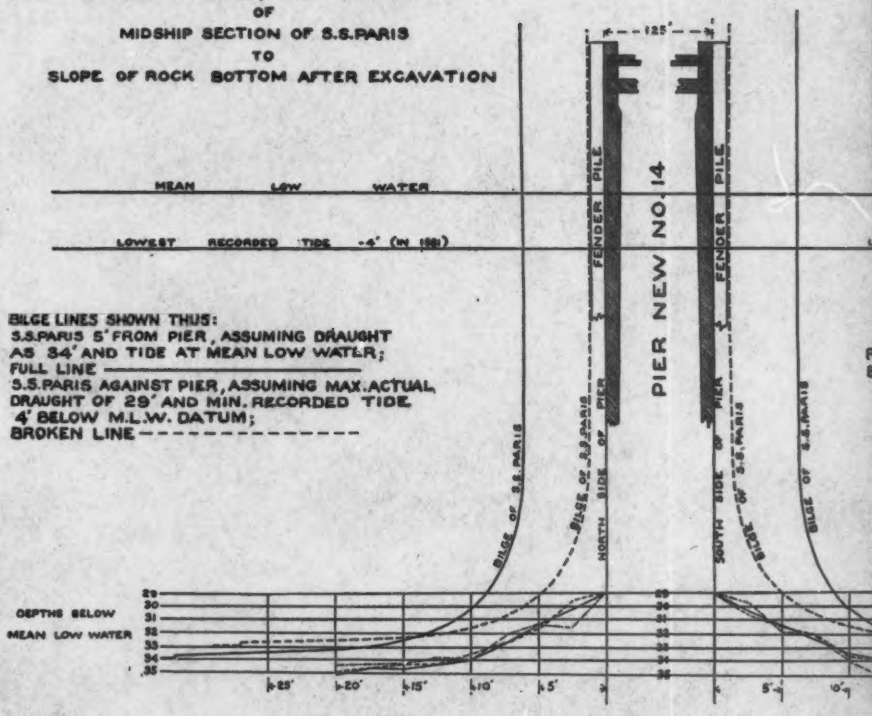

PLATE XXXV.
TRANS. AM. SOC. CIV. ENGRS
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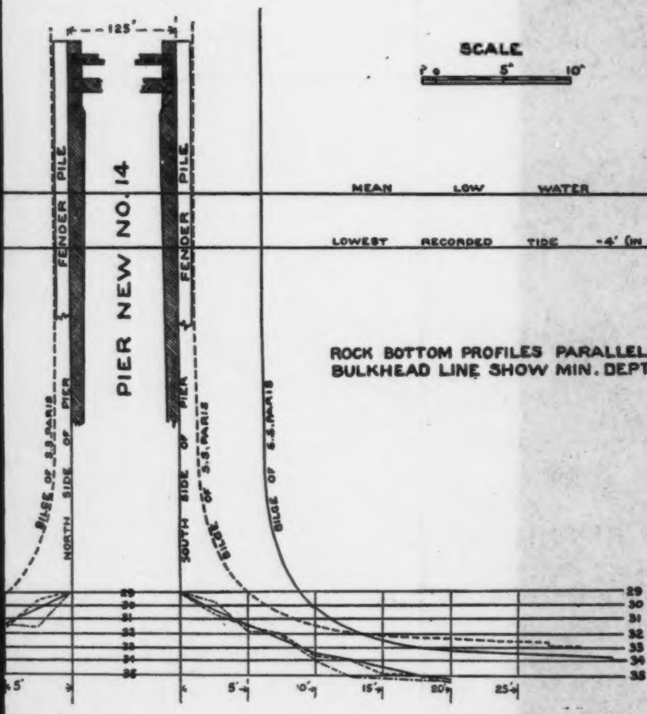
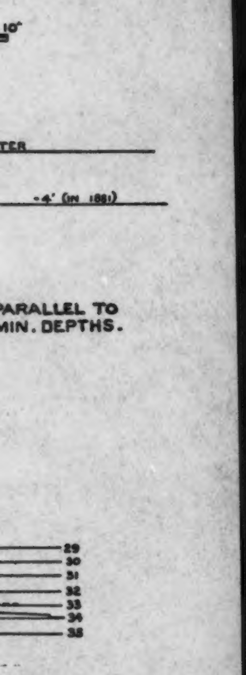
BENSEL ON SUBMARINE ROCK EXCA



XXV.
CIV. ENGRS.
No. 729.
ROCK EXCAVATION.



There were only three days when ice absolutely prevented the continuance of operations.

The first intention as regards part of this work was to have it done by contract; and the reef south of the outer end of the pier (see Fig. 2), being the one discovered first, was let, after asking for prices, to E. R. Lowe, a submarine contractor and wrecker, at a lump sum price which amounted to about $25 per cubic yard of rock (measured in place). Work was commenced under this contract on July 25th, 1892. The plant the contractor used consisted of two platforms which had not

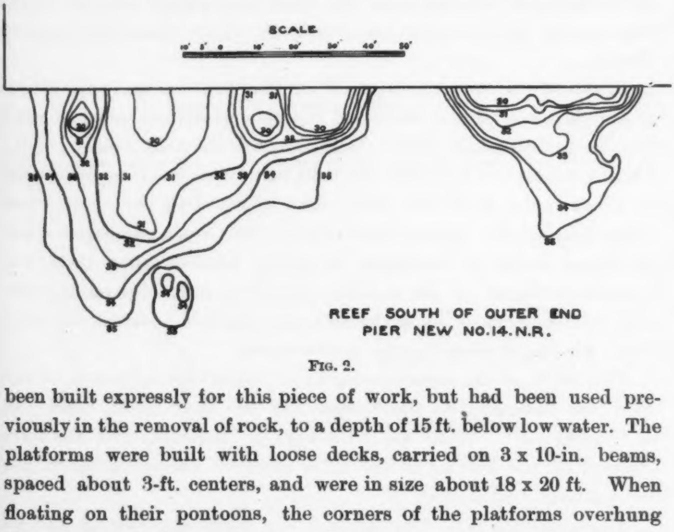


FIG. 2.

been built expressly for this piece of work, but had been used previously in the removal of rock, to a depth of 15 ft. below low water. The platforms were built with loose decks, carried on 3 x 10-in. beams, spaced about 3-ft. centers, and were in size about 18 x 20 ft. When floating on their pontoons, the corners of the platforms overhung the corners of the pontoons, and at each were boxes braced with knees to the two side timbers of the platforms. Through these open boxes passed 8 x 8-in. yellow-pine spuds 55 ft. in length, forming the standards or legs of the platforms when they were in position and ready for work.

These platforms were floated into position on pontoons and then the spuds were dropped through the boxes, by means of a floating derrick. After the spuds had been thus placed, differential pulleys were fastened to the tops of the spuds and to the corners of the platforms, and by means of these pulleys the platforms were raised from their pontoons which, when thus cleared, floated from beneath.

In general, the platforms were located about 3 ft. above high water. It will thus be seen that when located over the low portions of the reef, they stood on 8 x 8-in. legs, over 40 ft. in length, and without bracing of any description. It was found that the platforms shifted up and down stream, according to the flow of the tide, about 3½ ft. from the true position when the legs were vertical. The drills used by Lowe were of the largest size of the Ingersoll & Rand make, and were the same in size as those afterwards used by the Department. No uniform size of drill bar was used, as all the material was old stock, their size varying from 2 to 3 ins. in diameter. All the drills were turned down so that they fitted into 1½ ins. diameter standard chucks.

In almost every case the required length of drill was made up of three or four separate pieces connected together by double-ended chucks. In order to drill a hole deeper than the feed of the drill, namely, 3½ ft., it was necessary to lengthen the drill bar. This was sometimes done by means of the diver, but more often by replacing the whole extent of the drill bar by means of the derrick. This was a cumbersome and dangerous operation, especially in windy weather, when there was considerable swell on the exposed side of the pier. Operations were only just commenced when the first platform was wrecked, the swell from a passing steamer having overturned it.

The outfit at the commencement of operations consisted of one diver, one foreman, two blacksmiths and five deck hands, besides E. R. Lowe himself. While the drills were in operation, with the force enumerated, the drilling of the holes averaged about 13½ lin. ft. per day with one drill at work.

All the drilling was done without any particular system, and no plans were kept of the location or the depth of the holes drilled, except by the Department (see Fig. 3).

The party doing this work made an effort at first to connect the holes drilled and charged by means of the diver on the bottom. The effort was not successful, however, and three days were wasted, without any result except the losing of the holes. Afterwards all connections between the wires leading to the holes were made on the surface of the water. When blasts were fired, powder and tools which were kept on the floating derricks were moved about 100 ft. away from the area blasted. Large amounts of powder were used in the first opera-

tions on this reef, as the pier not having been widened at the time the work of blasting was going on, it was possible to use much more powder than was found safe in later operations which came close to the widened pier. In one case 300 lbs. of Hecla powder, which is said to contain 40% of nitro-glycerine, were fired at one blast. A perceptible shock was felt on the pier 200 ft. away, and the water was thrown about 40 ft. in the air. As large blasts as this were not allowed again.

After the platform had been wrecked for the second time, two platforms were set up and lashed together, and an effort was made to run four drills on these two platforms at the same time. After a trial,

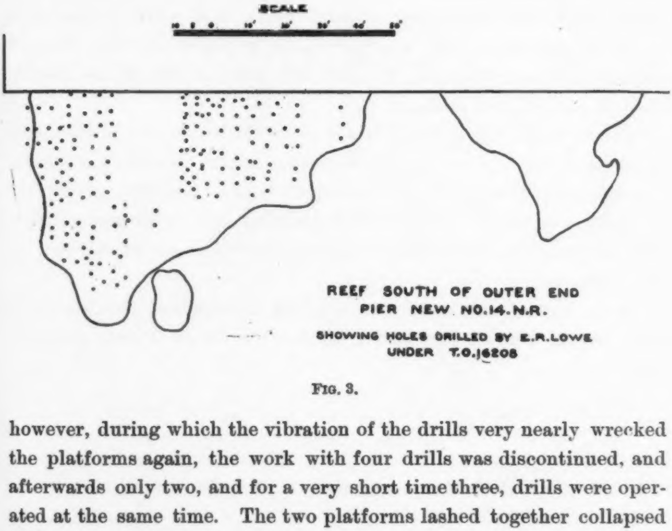


FIG. 3.

however, during which the vibration of the drills very nearly wrecked the platforms again, the work with four drills was discontinued, and afterwards only two, and for a very short time three, drills were operated at the same time. The two platforms lashed together collapsed suddenly, on a Sunday, when no work was being done, making the third collapse. The cause of this accident could not be determined. Two of the spuds were broken in this accident, and over six days were required to clear up the wreck. While the platforms were being repaired, some of the blasted rock was removed by means of divers, about 85 cu. yds., measured on the scow, being removed, with a daily average of $3\frac{4}{10}$ cu. yds., the force employed averaging ten men and two divers.

This work being considered too slow, the small dredge *Hussar* was placed at work dredging the blasted rock. This dredge had been built expressly for the recovery of the supposed sunken treasure that went down in the British ship *Hussar*, in Long Island Sound, during the period of the Revolution. The dredge was built to operate in very deep water, and was said to have dredged successfully in water 200 ft. deep.

The bucket of the dredge had a capacity of about 2 cu. yds., and was opened and closed by hydraulic power, a small rubber hose carrying the water from a pump on the scow to a hydraulic cylinder which formed part of the bucket. After a trial this dredge was found to be not adapted to the dredging of the blasted rock. The bucket being light and also being without poles, and more or less top-heavy on account of the heavy cylinder situated near the top and hinge of the bucket, caused it to fall over after it struck the uneven blasted rock on the bottom.

This dredge worked continuously from November 7th to 15th, and in that period excavated and removed about 116 cu. yds., scow measurement, or an average of about 18 cu. yds. per day. After this dredging was finished drilling and blasting were recommenced and continued to December 28th, 1892, when the work was abandoned by the contractor.

In all, during the period from July 25th to December 28th, 164 holes were drilled (Fig. 3) aggregating about 1 050 lin. ft. of 3-in. diameter hole.

Rock removed by divers	85	cu. yds.
" dredge	116	"
Total removed by contractor E. R. Lowe.	201	"

All the powder used in the above work was of the grade known as 40% Hecla. All holes were charged with this powder in 2½-in. diameter tin tubes. Efforts were made to drill all the holes 3½ ft. below the 35-ft. grade required. This, however, the writer does not think was done, except in a few cases. Judging from the result of subsequent blasts made by the Department, it is his opinion that a large amount of the powder used was simply placed on the top of the rock. No means were taken to keep an accurate location plan of the holes. In fact, all

DEPARTMENT OF DOCKS DRILL FLOAT
USED IN SUBMARINE ROCK REMOVAL
PIER NEW NO.14 N.R.
N.Y.

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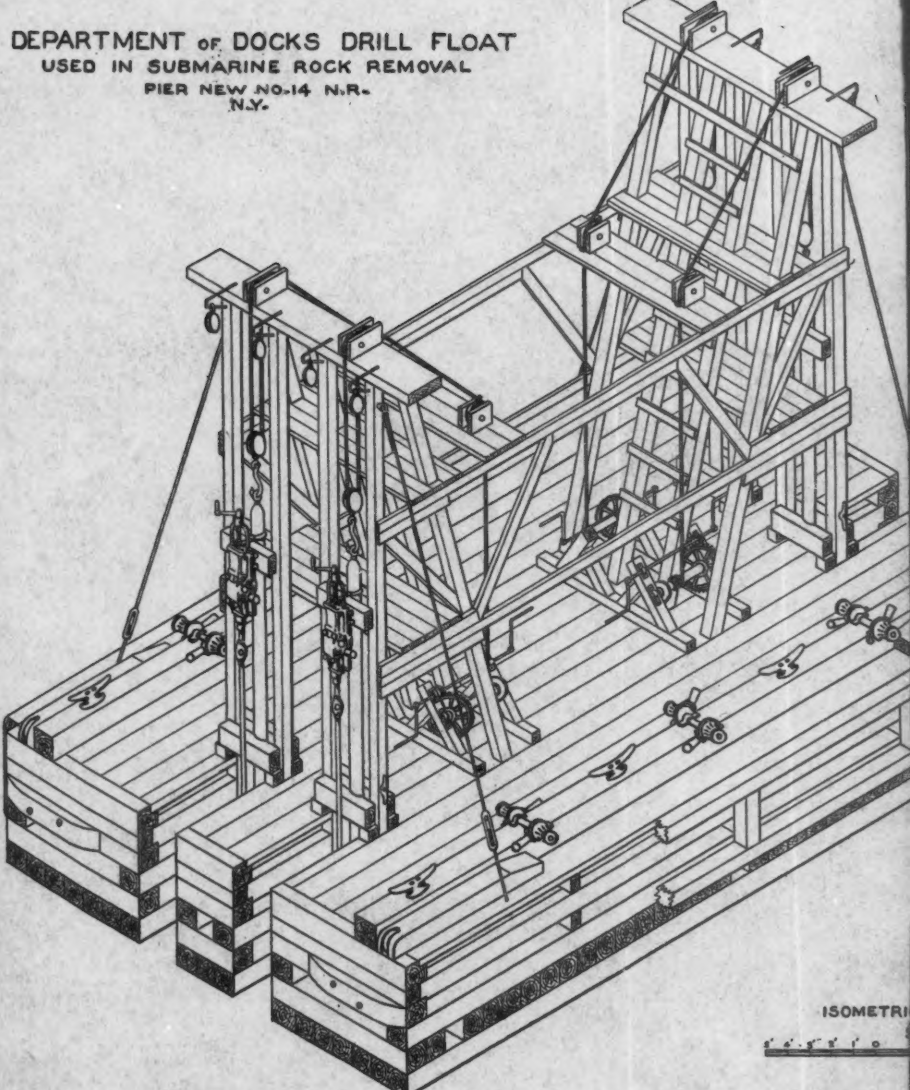

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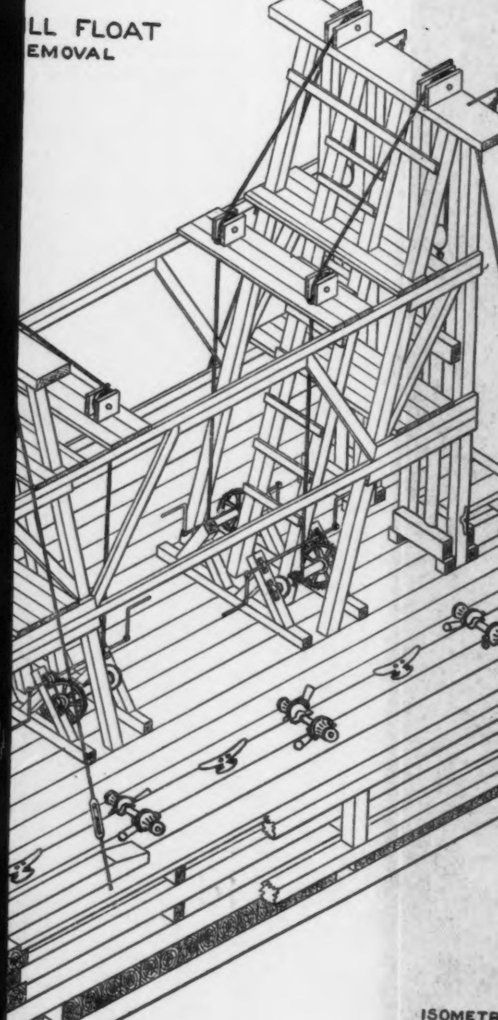

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PLATE XXXVI.
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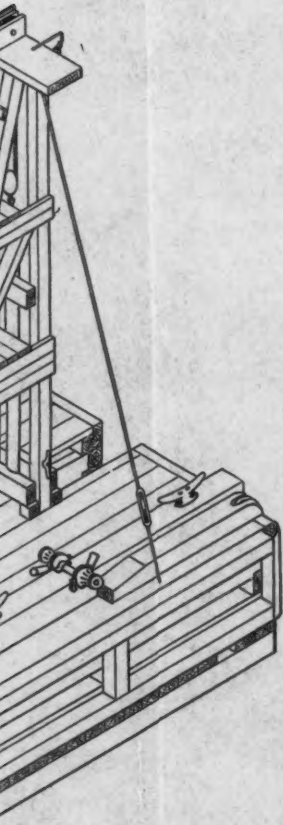
BENSEL ON SUBMARINE ROCK EXCAVATION.



ISOMETRIC PROJECTION

SCALE

of this work depended on the intelligence and honesty of the divers, and the men employed in this capacity were very inefficient.

At the time just before this work was abandoned, Mr. Lowe informed the writer that he had actually spent more than $8 000, which was more than the total lump sum bid for the work. The discouragements consequent upon the failure of the plant that was used, and the coming of the ice season, when it would have become absolutely impossible to use the platforms, led to his abandonment of the work.

Dock Department Plant.—When it became evident that the work of the removal of the reefs around Pier No. 14 (new) could not be finished by the contractor who had taken the work on one reef, the Department took the matter in hand, and constructed the drill float (Plate XXXVI) for further operations, it having been decided to remove the reef on the north side of and near the outer end of the pier, and the one lying south of and close to the inner end of the pier, by Dock Department labor. A single-drill float had previously been used in the removal of rock to a grade of 14 ft. below mean low water on the East River. The governing idea in the construction of the large four-drill float was to use as much as possible the class of material which, when it should be taken apart, could be used again in the bulkhead wall construction. With this end in view 12 x 12-in. spruce timber was used in the construction of the float itself. This timber was found to be exceedingly buoyant, and from actual trial the buoyancy of 1 cu. ft. was found to be about 33 lbs. After the operations of removing the rock were completed and this drill float was taken apart, the timber was not found to be at all water-logged, although it had been continually immersed in water for more than one year. The dimensions of the drill float were about 22 x 33½ ft., deck measurement, and 6 ft. in depth. The construction of this float will be readily seen in Plate XXXVI the whole apparatus being practically floated on the two bottom courses of timber. Four drills were located on the float, operating through well holes, each of the drills being capable of drilling a hole 12 ft. in depth without changing the drill or the drill bar. The drills were raised and lowered in ways by two men operating a hand winch. Two Ingersoll and two Rand drills were used, the drills being fastened to a 6-in. cast-iron column, by means of the standard mining attachments, with only a few additions to enable the drill to be kept in a vertical position without turning. In drilling a hole the drills

were fed by the hand-feed until the feed was run out, when the winches were called into requisition, the drills lowered, the hand-feed raised, then operations continued as before.

It was thus possible with this machine to drill the holes to the required depth, without change in the drill or drill bar, unless for other reasons than the deepening of the holes. In order to hold this drill float steady against wave action, and also against tidal rise and fall, four anchors were used, which consisted of ordinary pile-driver hammers, weighing about 3000 lbs. each. The drill float was accompanied by the standard Dock Department 12-ton derrick, containing two 40 H.P. boilers, and having a single mast with two booms. In locating the drill float, the derrick lifted the four pile-driver hammers, the hammers being rigged in couplets, two for each end of the float. After the hammers were clear of the bottom, the derrick and float alongside were floated into the desired position over the reef, and the hammers were let go and cast free from the booms of the derrick. Other lines from these hammers, or anchors, which ran over the pulleys at the corners of the drill float, were then made fast to the hand winches, and the whole drill float hove down by means of these winches, until the deck of the float stood about 1 ft. or 18 ins. above the surface of the water. Connections now being made from the boilers on the derrick to the steam rock drills, the operation of drilling could commence from what had become a fairly stable structure.

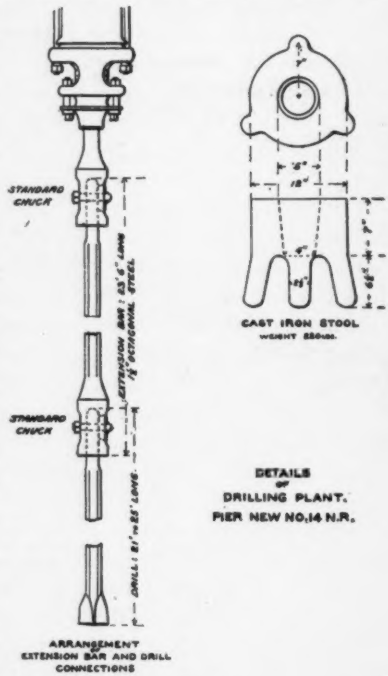


Fig. 4.

PLATE XXXVII.
TRANS. AM. SOC. CIV. ENGRS.
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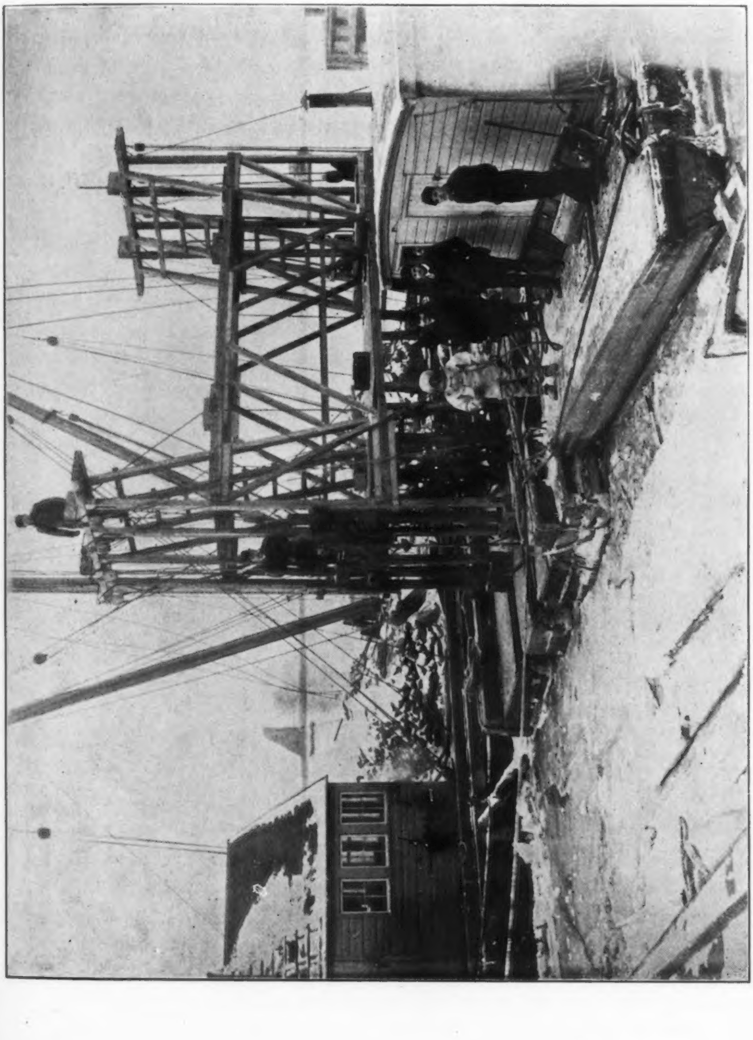
BENSEL ON SUBMARINE ROCK EXCAVATION.



Where these operations were conducted, and during the period of time which they covered, there was considerable high wind and a large amount of ice, the waves oftentimes rising to a height of over 3 ft. along the side of the pier, and washing over the deck of the float. At one period, about the latter end of December, this drill float operated while surrounded entirely by ice over 6 ins. in thickness. Plate XXXVII shows the drill float at this time.

A single-drill float was used entirely on the reefs south of the inner end of the pier, and on the one south of the outer end of the pier, when the Department finished up and completed the work which had been abandoned by the contractor.

The drill bars used by the Department (Fig. 4) consisted of two pieces of octagon steel 1½ ins. in diameter, and when joined together made a total length of 45 to 50 ft. The two pieces were ordinarily of about equal length, one piece, the extension bar, having welded at one

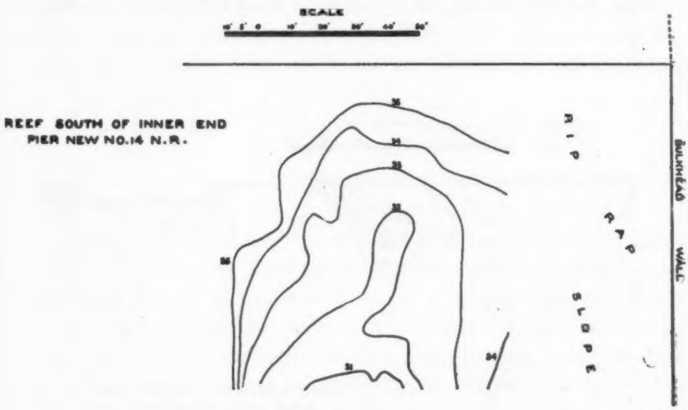


FIG. 5.

end the ordinary standard drill chucks, and the other end being turned so as to fit into the chuck of the drill; the lower length of the drill bar formed the drill proper, one end being fitted into the chuck on the extension bar, and the other end being upset and formed into a drill point with a diameter of 3 ins.

In drilling an 8-ft. hole the diameter of the drill decreased to 2½ ins., and sometimes, in especially hard rock, to 2¼ ins., diameter. Toward the close of the work, when the holes in the chucks had be-

come considerably worn, difficulty was found in making the connections tight, both at the drill and at the end of the extension bar, and had the work gone further, a more positive chuck, not depending on the friction of a key, would have been made. The drills were operated with a steam pressure of 95 lbs. per square inch at the drill.

The locations of the reefs made the conduct and procedure of the work different on each of the various areas. On the reef south of the inner end of the pier (Fig. 5) mud formed so quickly that it was found impossible to clear off with the pump and to have the reef remain clear of mud long enough to permit the drilling of any number of holes before blasting them. The reef north of the outer end of the pier, however, was kept entirely free from mud by the current, and we were enabled to drill about half the area of the reef with holes before commencing the operation of blasting. On the reef south of the outer end of the pier, after the work had been abandoned by the contractor, the Department put a dredge to work and cleared the area of all the loose rock. The reef was then found to be in the condition shown in Fig. 6. The top surface of the rock being soft and seamy,

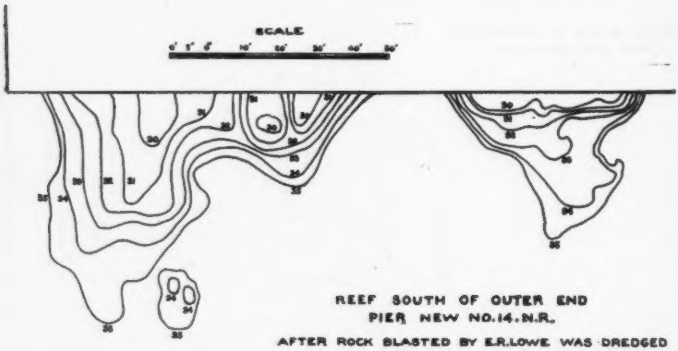


FIG. 6.

considerable difficulty was found in drilling holes in this reef, and a large portion of the rock was removed or reduced to powder by surface blasting. The small portion of this reef lying inshore of the main body of the reef on the south side of the pier was removed entirely by surface blasting, only a small depth of rock having to be removed.

Pier No. 14 was widened by the addition of a 50-ft. strip to the southerly side, making it, as it now exists, 125 ft. in width instead of 75 ft., as formerly. This work of widening had been carried on during the time the contractor had been operating on the reef south of the outer end of the pier, and at the time that the Department commenced operations this work had been nearly completed and a crib 30 × 50 ft. sunk at the extreme outer end of the pier, where it was impossible to drive piles. The effects of the surface blasts on this crib were quite remarkable. At one time a blast of about 15

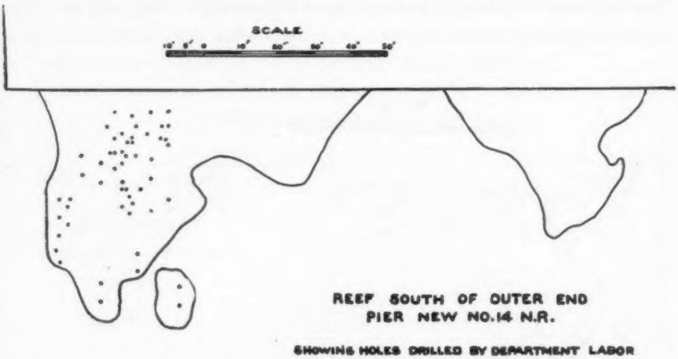


Fig. 7.

lbs. of gelatine, located about 15 ft. south of the crib when fired, threw it in the air about 1 ft. And as the crib was located on a rock bottom which shelved out shore, each firing of a surface blast threw it further out in the stream. Holes were drilled in the rock close to the outer edge of the crib on the bottom, and four drill bars placed in the holes, bearing against its outer timber. After it had brought up solidly against these bars, no further movement was made by the blasts.

The Department drilled 53 holes in this reef (Fig. 7) after taking hold of the work, and, in addition, by the use of a considerable amount of powder in the way of surface blasts, the reef was removed by the dredge to the condition shown in Fig. 8, and then the remainder of the rock taken out by divers and the use of surface blasts.

The Department commenced the operations on this reef on March 18th, and finished the work on October 26th, 1893.

On examination made by divers, the entire area of the reef south of the inner end of the pier was found to be covered with from 5 to 7 ft. of mud, soft on top, but quite firm and solid for the bottom 3 ft., although this reef had been dredged to the solid rock only about a week previous.

Operations were commenced on this reef by trying to clear it of mud, a 10-in. rotary pump on the derrick being used. As an experiment, a trench was dug about 20 ft. in width, running from east to west across the entire extent of the reef, or up to about the bottom of the rip-rap slope which forms part of the bulkhead wall construction. The pumping of this trench required six consecutive days, after which the

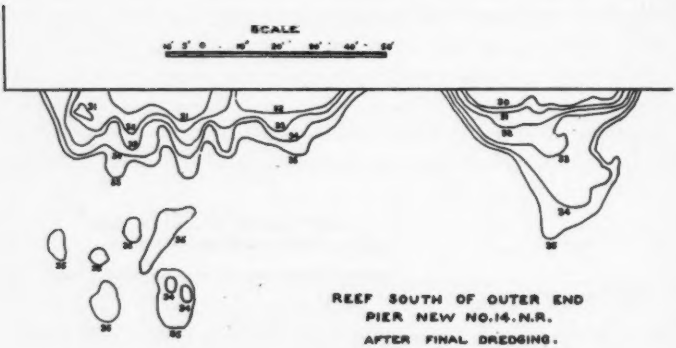


FIG. 8.

drilling was commenced with the single drill float. After drilling for about one month, during which time 76 holes had been drilled, and about half of them blasted, it was found that the mud was forming so rapidly that there was danger of losing those holes that had not been blasted, in spite of the fact that all of them had $\frac{1}{2}$-in. iron rods in them which projected about 2 ft. above the surface of the rock.

No more pumping was done on this reef before the drilling, but the holes were afterwards drilled through the mud, and the only pumping that was necessary was done where the mud had become so deep as to prevent the diver loading the holes that had been previously drilled.

The attempt was made on this reef to charge and fire the holes soon after they had been drilled, and not to have any large number of them drilled in advance of the blasting. This was done in order to save the

holes and to save time in finding them through the deep mud. But it resulted in the loss of quite a number of them by their being destroyed by contiguous blasts.

Rack-a-rock powder was first used on this reef, the charges being placed directly in the holes without the intervention of tin tubes, the holes where this powder was used being situated in that portion of the reef that was first pumped clean.

Difficulty arose in the use of this powder, however, the divers claiming that it did not throw out the rock, and quite a number of charges burning without exploding. Where this occurred, the cartridges containing the exploders either partly or entirely burned and the remaining cartridges or sausages had to be removed by the diver.

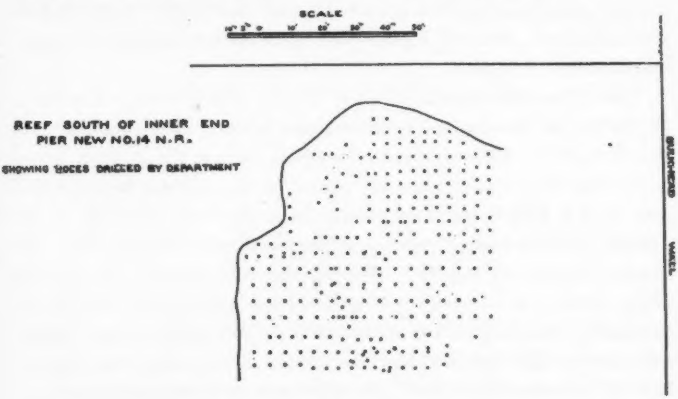


FIG. 9.

After 420 lbs. of this powder had been used, the use of nitro-glycerine powder was commenced. That known as forcite powder, containing, it is stated by the maker, 60% nitro-glycerine and 40% of a patented mixture of wood pulp, gun cotton and nitrate of soda, was the first used. The burning of the powder in the hole occurred afterwards with the forcite powder, but it was traced to the use of poor exploders that had been bought in haste and had probably been kept in stock some time.

After using a small quantity of this 60% forcite powder, what is known as forcite gelatine was used, and nearly the entire remaining work done by the Department was with this powder. Gelatine is

claimed to contain 95% of nitro-glycerine and 5% of gun cotton and some other absorbent. It was claimed to be extra safe on account of the difficulty of exploding it, the makers claiming that this difficulty was so great that the exploders should always be placed in a cartridge of the lower grades, say, 40 or 60% powder. There was no difficulty, however, when a double-strength exploder was placed in the gelatine itself. In the use of this powder, however, it was found that a weak exploder would simply set it burning, with slow combustion, a dark, reddish gas being evolved, which smelled not unlike chlorine. In some of the holes the entire charge burned out in this way, while in others the cartridge containing the exploder, and sometimes only a portion of this cartridge, would burn. All of these charges that burned slowly in this way occurred in the reef south of the inshore end of the pier, and with a particular lot of exploders that were afterwards found to be poor.

One of the difficulties in the use of this nitro-glycerine powder is the fact of its freezing and becoming unreliable at a temperature lower than 42° Fahr. This necessitated keeping a fire in the powder house.

In this case a small range was placed in the powder house, which was a 10 x 10-ft. structure placed on a scow and covered on the outside with tar-roofing paper, and the temperature inside kept constantly between 65 and 75°. During the cold weather the powder always arrived in a frozen condition, and the thawing out was done by immersing a galvanized iron bucket filled with the gelatine cartridges in a tub containing water that had been heated on the range to a temperature of not above 100° Fahr. There seems to be considerable doubt as to what is the dangerous temperature of this powder, some people who had used the gelatine claiming that it would stand 200°. No difficulty, however, arose in its use, limiting the temperature as stated, and the powder never became so soft as to run and become unmanageable. Using the bucket and the water in this way none of the cartridges ever came in contact with either the hot water or any hot metallic surface. Owing to the large amount of handling which has to be gone through with in using the powder which has become frozen, the cartridges, which were 2½ and 2¼ ins. in diameter and 8 ins. long, were wrapped carefully in extra heavy paraffine paper before being sent from the factory, and this paper was never opened, except in the case of the one cartridge into which the exploder for each charge was inserted.

The drilling was commenced on this reef, south of the inshore end of the pier, on September 2d, 1892, and continued without interruption, except for blasting, until March 13th, 1893, during which time 231 holes were drilled (Fig. 9), aggregating about 162 lin. ft. all the holes being 2½ and 3 ins. in diameter, and having been drilled 3 ft. below the 35-ft. grade. The first dredging was done from March 13th to 22d, inclusive, 703 cu. yds. of mud and rock being taken out, the dredge being used with a scoop. After the dredge had taken out all that

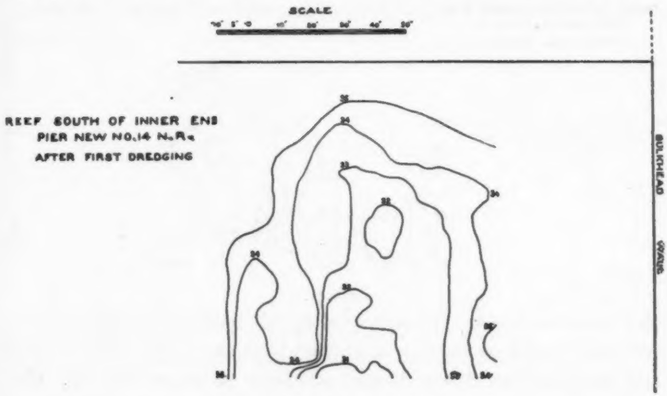


FIG. 10.

seemed possible (Fig. 10), an examination by divers showed that there was still much loose rock on the bottom, and also some pieces that had been detached from the bed-rock, which lay too close together for the dredge to remove. After the dredge was through this first dredging, 15 holes were drilled and fired, and a number of surface and seam blasts were set off; after which, the dredge was turned in again. Altogether, the dredge was at work eight separate times on this reef; and between each time drilling and blasting holes, also, seam and surface blasts were set off, to loosen the rock.

The condition after final dredging is shown in Fig. 11.

In all, there were 79 holes drilled and blasted between the time that the dredge first commenced work, and the time when the removal of the reef was finished. During the same time 86 cu. yds. of loose rock that had escaped the dredge were taken up by divers. The work on this reef was finished on November 2d, 1893.

Operations were commenced on the reef north of the outer end of the pier (Fig. 12) on October 19th, 1892, when an examination by divers disclosed the fact that, although the reef was pretty fairly clear of mud, it was encumbered by a quantity of rip-rap. About 30 yds. of

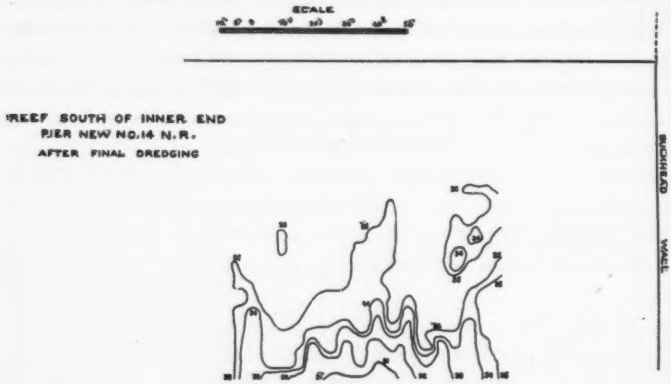


FIG. 11.

this were removed by the divers before the drilling commenced. The reef was covered on the top, in addition to the rip-rap, with some sand and hard-pan, and it was deemed advisable to pump this off. This operation proceeded so slowly, however, that it was abandoned, and actual drilling was commenced on November 11th, 1892.

As stated before, an endeavor was made in this reef to drill the holes over one-half the whole area before the blasting operations were commenced; exception, however, was made when the necessity for repairs compelled the abandonment of drilling, and also where holes were placed close to the side of the pier, and there was danger of losing them on account of the tumbling down of the rip-rap under the pier. At these times blasting was done, as became necessary.

The rip-rap under the pier, which became very troublesome later, was placed to a grade of 15 ft. below mean low water, under the pier, sloping down to a grade of 20 ft. at the side of the pier. Every blast dislodged some of this rip-rap, and it is estimated that on the reef north of the outer end of the pier there was removed, besides the solid rock, about 400 yds. of it, a considerable portion of which, existing, as it did, close to the side of the pier, was about as expensive

to remove, either by the diver or the dredge, as was the blasted rock itself.

The idea in drilling the large number of holes in this reef before doing any blasting was to try to escape the delay and expense found to occur on the reef south of the inshore end of the pier, where new holes had to be drilled close to where the holes had previously been blasted, and the rock had been left shattered and in seams, and yet not loose enough to be removed by the dredge. Often when a drill struck any of these seams, the hole had to be abandoned and a new one sunk near it.

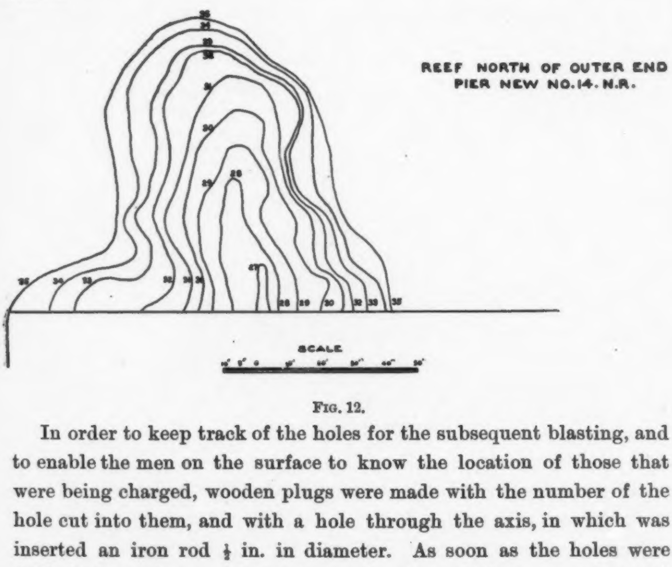


FIG. 12.

In order to keep track of the holes for the subsequent blasting, and to enable the men on the surface to know the location of those that were being charged, wooden plugs were made with the number of the hole cut into them, and with a hole through the axis, in which was inserted an iron rod ½ in. in diameter. As soon as the holes were drilled, these plugs, with the iron rods through them, were inserted in the holes in the rock and tamped firmly in place by the divers. In the charging of the holes afterwards a diver was dropped down about over the spot where the holes to be charged were supposed to be located, and he removed the plugs of the nearest holes and sent them to the surface. It could then be readily seen whether he was located on the bottom correctly, and also whether he was charging or cleaning out the holes intended.

After drilling and blasting the holes on the westerly half of the

reef the drilling was done on the easterly half, the holes being commenced at the easterly edge and worked westerly, and located about as shown in Fig. 13. It was found that the force of the tide had considerable influence on the location of the hole on the bottom, and examples were found with a variation of 3 ft. between the location of the drill on the surface and that on the bed-rock below. For almost the entire first series of holes, drilled before any dredging was done, no stool or guide for the drill on the bottom was needed or used, and the long-drill steel vibrated in a circle of probably 4 ft. in diameter on the bottom under the influence of the strong tidal current before it drilled sufficiently into the rock to guide itself. This, of

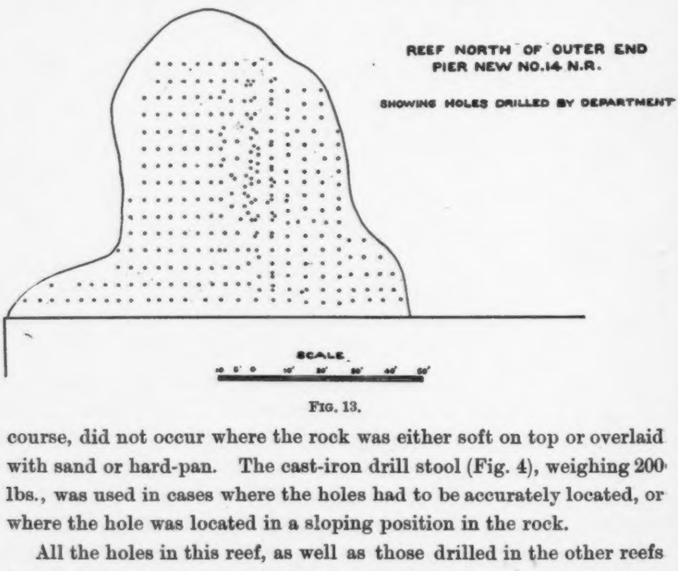


FIG. 13.

course, did not occur where the rock was either soft on top or overlaid with sand or hard-pan. The cast-iron drill stool (Fig. 4), weighing 200 lbs., was used in cases where the holes had to be accurately located, or where the hole was located in a sloping position in the rock.

All the holes in this reef, as well as those drilled in the other reefs by the Department, were drilled to a grade of 38 ft. below mean low water, except close to the sides of the pier, where the bottom was sloped up, as shown in Plate XXXV. In some cases, where redrilling was done over areas of the bed-rock where it had formerly been drilled, the rock was so disturbed by the previous blasting that it was impossible to get powder down to the bottom of the hole. In almost every case, however, the explosive was placed down as far as 35 ft. below

mean low water. After all the regularly spaced holes located (Fig. 13) were drilled and fired, the dredge was put to work on March 22d, working from that date until the 28th, and excavating and removing in that period 229 cu. yds., scow measurement.

Soundings were then taken, and the reef found to be in the condition shown by Fig. 14, which plainly shows a ridge through the center of the reef nearly as high as the original grade.

An examination by divers disclosed the fact that, although the rock was broken and seamed where the grade was high, the broken stone lay in such a position that the dredge skimmed over the top and did not penetrate into the mass. Charges were prepared, and wherever a seam could be found, a charge was placed and fired. After this was

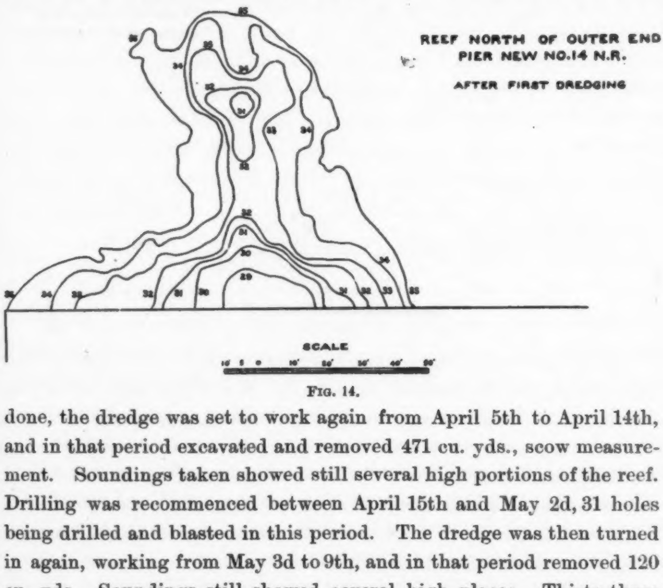


Fig. 14.

done, the dredge was set to work again from April 5th to April 14th, and in that period excavated and removed 471 cu. yds., scow measurement. Soundings taken showed still several high portions of the reef. Drilling was recommenced between April 15th and May 2d, 31 holes being drilled and blasted in this period. The dredge was then turned in again, working from May 3d to 9th, and in that period removed 120 cu. yds. Soundings still showed several high places. Thirty-three holes were drilled and blasted before the dredge was put to work again. This trip of the dredge succeeded in removing only about 100 cu. yds. in four days. The divers reported much loose rock, part of which could be felt from the surface. A trial was made to see what could be done with divers, and in one day 6 cu. yds. were re-

moved. The reef was now in fair shape, except close to the pier, where it was impossible to use the dredge. After drilling 29 holes, blasting a number of seams and firing a few surface blasts, the dredge was put to work again on June 6th, and between that date and June 12th, 152 cu. yds. were removed. Thirty-six holes were then drilled, after which the dredge took out 113 cu. yds. in three days. After drilling 33 holes and blasting them, where the rock was a very small amount above grade, the dredge was put to its final work on this reef on July 24th, and from the 24th to the 28th removed 100 cu. yds. Soundings taken after this showed the reef in the condition shown in Fig. 15.

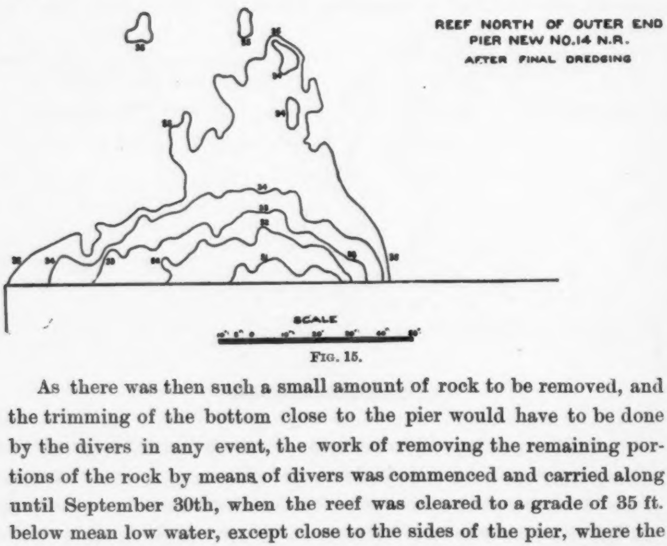


Fig. 15.

As there was then such a small amount of rock to be removed, and the trimming of the bottom close to the pier would have to be done by the divers in any event, the work of removing the remaining portions of the rock by means of divers was commenced and carried along until September 30th, when the reef was cleared to a grade of 35 ft. below mean low water, except close to the sides of the pier, where the rock was trimmed to fit the bilge of the American line ships *Paris* and *New York*, as shown in Plate XXXV.

The nature of the rock in all these reefs was about the same, consisting of gneiss and mica schist seamed with quite extensive veins of quartz. This made both the drilling and the blasting difficult, as the soft mica rock in some places was extremely difficult to remove by surface blasting, even when only a small height was to be knocked down.

The holes drilled by the Department in all the reefs for the first series were spaced 6 ft. apart in northerly and southerly direction, and 5 ft. apart easterly and westerly. The holes being drilled in a vertical direction, the result of the blasting seemed to be to stack the broken stones against each other, and not to loosen and throw up the mass. Divers described this bottom as being often times like a pile of gravestones, one stone lying against another.

Dredging.—The dredging of the blasted rock was very disappointing. All kinds of buckets were used, including the scoop, clam-shell buckets in two different sizes, 4 and 7 yds. capacity, and the grapple. There was little or no perceptible difference in their effects. On the first day the dredging was done on the reef south of the outer end of the pier; fairly satisfactory work and progress were made, 100 cu. yds. being excavated and put into the scow. The writer thinks it probable that this progress was possible because E. R. Lowe had made a large number of surface blasts, the charges being supposed to be placed in holes, and the rock was powdered pretty well all over the top of the reef, and no difficulty was experienced in its removal. The number of hours that the dredge was employed on each separate reef is as follow:

Reef north of outer end	389 hrs.	25 min.
Reef south of outer end	266 "	50 "
Reef south of inner end	230 "	05 "

In one case, directly after the dredge had been stopped because it was doing work of little or no consequence, 30 cu. yds. of stone were sent up by the divers, who simply loosened the stone on the bottom with their bars, the broken stone having formerly lain in such a position that the dredge buckets skimmed over the top. It would seem as if part of the explanation of the slow work done by the dredge lies in the fact that the rock would be best operated on by a machine using the scoop, and in the fact that there is no machine, so far as the writer is aware, certainly none in this vicinity, capable of operating a scoop in a 40-ft. depth of water. Extensive alterations would have to be made in the rig of the machine, in order to get a greater horizontal thrust to the scoop. The dredge employed on this work was one of the largest in this harbor, but to rig it to use the scoop, or dipper, for the depth required, simply meant the extension of the dipper handle, with no increase in the overhang of the boom, which would cause an

increase in the vertical component of the force exerted by the chains, and a consequent decrease in the thrust or horizontal component of the force.

This dredge, like others in the harbor, is built to operate and to dredge to a depth of 25 ft. below mean low water, but, though dredging may be done with it to a depth of 30 ft. in mud or sand, the increase to 35 and 40 ft., and the operations being confined to rock, were conditions too hard for it to master satisfactorily.

The work here described was done under the personal direction of the writer as Assistant Engineer of the Department of Docks, of which G. S. Greene, Jr., M. Am. Soc. C. E., has been for a long period Engineer in Chief.

The cost of the work divided into the several reefs is as follows:

Reef North of Outer End—	
Labor	$16 949 59
Material	3 395 42
Dredging	9 731 25
Towage	548 73
Reef South of Outer End—	
Labor	6 974 30
Material	1 816 84
Dredging	6 662 50
Towage	274 36
E. R. Lowe paid on account	3 798 67
Reef South of Inner End—	
Labor	13,217 16
Material	2 526 24
Dredging	5 752 08
Towage	548 73
Total	$72 195 87

The cost of building the two drilling floats is as follows :

Labor	$946 18
Material	4 101 50

The calculated amount of rock removed is as follows :

Reef north of outer end	766 cu. yds.
Reef south of outer end	357 "
Reef south of inner end	407 "
Making a total of	1 530 cu. yds.

of solid rock measured in place.

Besides this amount of solid rock, there was removed about 450 cu. yds. of rip-rap, thus making the total 1 980 cu. yds. of rock removed.

The amount of rock removed, measured in the scow, is as follows:

Reef north of outer end, by dredge	1 285	cu. yds.
" " " by divers	150	"
Reef south of outer end, by dredge	843	"
" " " by divers	118	"
Reef south of inner end, by dredge	2 677	"
" " " by divers	150	"
Making a total of rock removed and measured in the scow	5 223	cu. yds.

DISCUSSION.

J. F. O'Rourke, M. Am. Soc. C. E.—What kind of rock was that?

John A. Bensel, M. Am. Soc. C. E.—Gneiss, with veins of mica schist, and quartz.

T. Guilford Smith, M. Am. Soc. C. E.—I would like to ask about the $72 000 the Department spent in addition to the $8 000; the original estimate of the Department was about this, was it?

Mr. Bensel.—There was no estimate made; the $8 000 paid Lowe is included in the amount of about $72 000; as may be seen on page 252, bids were received, of which Lowe's was the lowest. The bids were lump sum prices, to make 35 ft. at low water on the reef south of the outer end of the pier.

Desmond FitzGerald, M. Am. Soc. C. E.—What has been the ordinary price in 25 ft. of water?

Mr. Bensel.—It varies greatly, probably between the limits of $35 and $11.

Mr. O'Rourke.—Did you distribute the expenses so you could tell how much the drilling cost?

Mr. Bensel.—I have not done so in this paper. The dredging is in the neighborhood of $20 000, the construction of plant was about $5 000, and the amount paid Lowe about $8 000. The remainder, after subtracting the above, is the cost of the drilling, blasting, sounding, tubing and pumping the rock.

Mr. Smith.—You have the plant left?

Mr. Bensel.—Yes, but probably no use for it in the near future.

Mr. O'Rourke.—Was there ever considered the possibility of drilling through the mud?

Mr. BENSEL.—As stated in the paper, most of the drilling where the mud occurred, on the reef south of the in-shore end of the pier, was done through the mud. In loading the holes, however, where the mud was more than 3 ft. in depth, it became necessary to pump, in order that the diver might operate.

Mr. O'ROURKE.—I mean without a diver.

Mr. BENSEL.—A trial was made in drilling through a wrought-iron pipe, but the length of the pipe, over 50 ft., and the consequent difficulties in its handling made the work slower and more expensive, and its use was therefore abandoned.

R. A. CUMMINGS, Assoc. M. Am. Soc. C. E.—Has the Lobnitz system been considered in doing this work?

Mr. BENSEL.—No bids were received from any one using that system.

Mr. CUMMINGS.—I am informed there is a machine to operate on this principle being built in New York for rock excavation?

Mr. BENSEL.—There is one in Troy, working on a Government contract. I doubt if it would have worked in this case with the hard rock that was met with.

Mr. CUMMINGS.—I would like to say for the information of the members that there is a dipper dredger in this country operating in a depth of 40 ft. of water. It was illustrated and described in the technical press some time last fall.

G. S. GREENE, Jr., M. Am. Soc. C. E.—Is that on shale?

Mr. CUMMINGS.—It is on hard material.

Mr. GREENE, Jr.—I think I know the work, but I do not think it has been worked yet at so great a depth as 40 ft.

Mr. CUMMINGS (by letter).—The dredge I referred to was designed for 40 ft. depth of working, and was built for the Harbor Commission of Montreal.

The depth to which the Montreal Harbor Works are being constructed is 27½ ft. at low water. Inasmuch, however, as in the spring of the year, the water frequently rises 15 ft. above low water, the dredges all have a capacity of going to a depth of 40 ft. in order to reach the bottom during high water. There are some dredges that can work to 41½ ft.

Mr. GREENE (by letter).—Mr. John Kennedy, M. Am. Soc. C. E., informs me that Mr. Cummings is correct, and that such dredges have been used for excavating shale at 40 ft.; and that he designed a dredge for excavating phosphate rock at 42 ft. depth for South Carolina.

I do not think that rock so hard as that referred to in Mr. Bensel's paper has before been excavated to a depth of 40 ft. below the surface of the water.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

730.

Vol. XXXII.—September, 1894.

ON FORM OF RAILWAY EXCAVATIONS AND EMBANKMENTS.

By D. J. WHITTEMORE, Past President Am. Soc. C. E.
READ AT THE ANNUAL CONVENTION, JUNE, 1894.

WITH DISCUSSION.

Nowhere in quiescent Nature do we find earth surfaces in right line planes, or changes in direction of same made by abrupt angles, yet it is the general practice in laying out and constructing railway excavations and embankments to establish slopes on such planes intersecting the natural surface of the ground and the formation level of the road-bed in a well-defined line.

These conditions are never, or rarely ever, maintained. Storm water breaks over the crest thus formed, seeking and making channels of least resistance down the unprotected slopes, bringing with it earth from the crest and from immediately below it, filling the ditches in excavations. Embankments suffer change of form from similar cause; and immediately after the line of railway is opened for traffic the ditching train is called into service to remove the slush from ditches and to widen embankments, at a cost of from $1 to $2 per cubic yard; and this expense is continued year after year, and charged to opera-

tion. Often this expense during the first year of operation will exceed the cost of forming and protecting the slopes of excavation on the plan shown by the diagram herewith, which, with the above remarks, the writer submits without further comment to the consideration and criticism of the members.

DISCUSSION.

Foster Crowell, M. Am. Soc. C. E.—In looking over this paper by Past President Whittemore, I have been impressed both by its brevity and its comprehensiveness. I have rarely seen so much contained in twenty lines. It hardly needs anything more.

But I can testify from my own experience that the views expressed in it, not only as to the adoption of modified slopes for railroad earthwork, but also concerning the general principle that the work of construction should be, so far as possible, complete, and not left in a condition to entail excessive expenditure in operation, cannot be too strongly impressed on engineers.

When my acquaintance with earthwork began, many years ago, the setting of slope-stakes was chiefly a mathematical exercise, entered into with great zeal and mistaken faith in the permanence of plane surfaces in new fills and cuts ; but I soon had an opportunity to learn the error of such ways, in laying out slopes in Fairmount Park (Philadelphia), so as to "stand," and to resemble Nature.

The experience which I gained there was afterwards carried into railroad building, and demonstrated the advantage of following Nature's forms as far as possible, and in the manner which Mr. Whittemore sets forth.

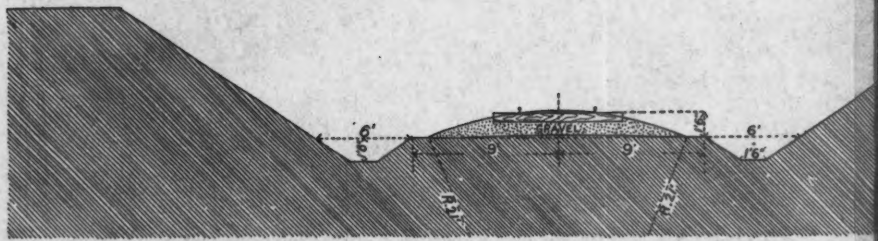
I also learned that it was a great deal better and cheaper in the long run to move material once for all and charge it to construction, than to handle a similar quantity several times a year thereafter and charge it to maintenance.

I might point out, further, that the desirability of flattening or rounding off the top of a cut is emphasized in cases where there are catchment ditches, which saturate the ground and increase the tendency to break away.

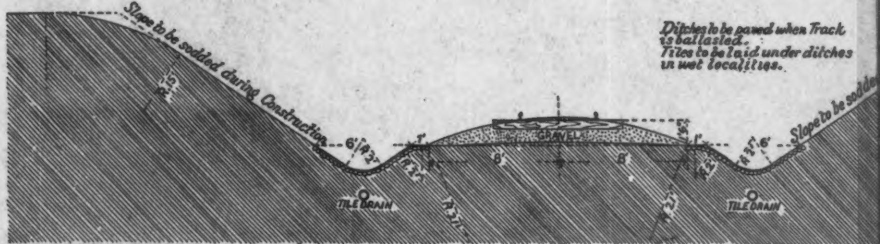
The toe of a rectilinear embankment, if left to itself, will come to a curve of stability; the shoulder will, at the same time, break down and slough away. Nature should be assisted at both points.

It not infrequently happens that excess material is wasted at the nearest available spot to save extra haul, while at other times, borrow

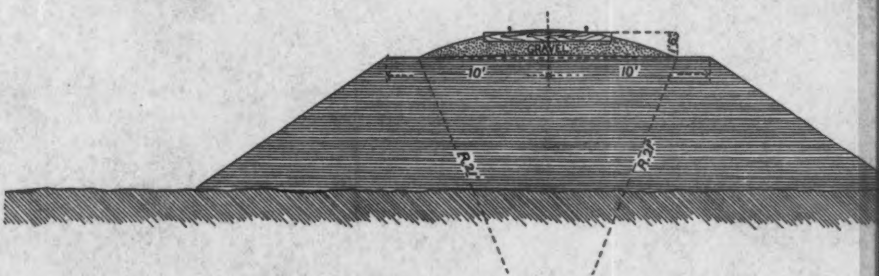
Customary Section of Roadbed in Excavation.



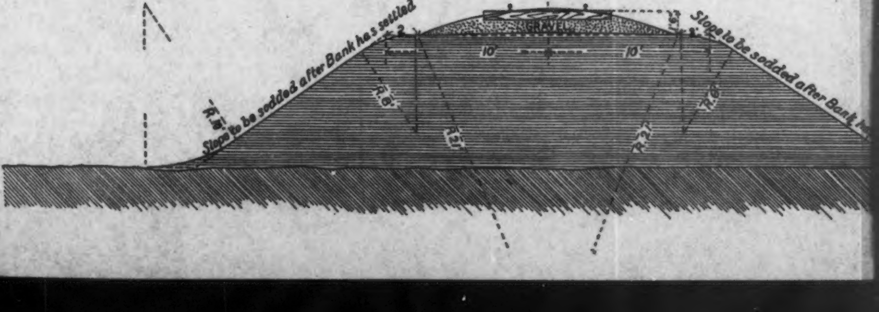
Proposed Section of Roadbed in Excavation.



Customary Section of Roadbed on Embankment.



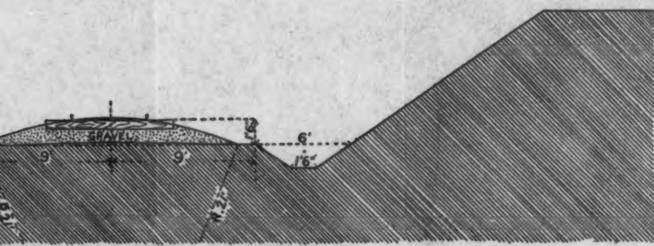
Proposed Section of Roadbed on Embankment.



Customary Section
6'
9'
R.21'
Proposed Section
ed during Construction
6'
R.3'
1'
6'
R.3'
TILE DRAIN
R.37'
Customary Section o
10'
R.21'
Proposed Section of Ro
to be sodded after Bank has settled
2
10'
R.6'
13'

PLATE XXXVIII.
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXXII, No. 730.
WHITTEMORE ON RAILWAY EXCAVATION AND EMBANKMENTS.

...ary Section of Roadbed in Excavation.



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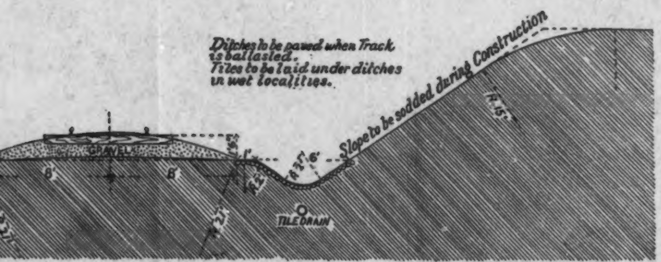
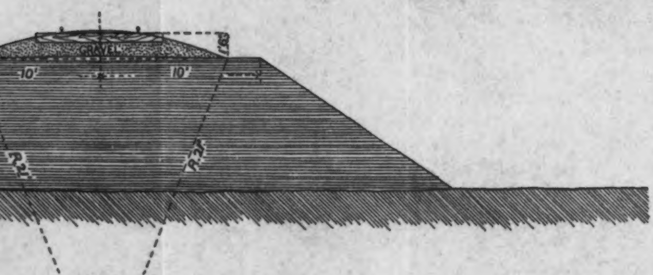
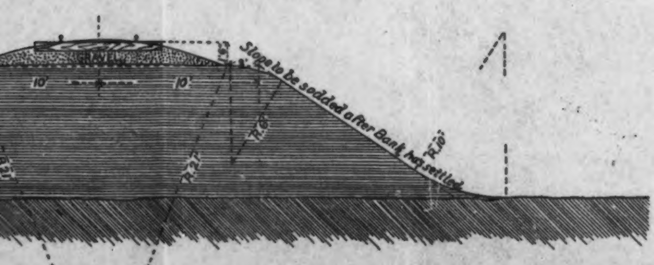


...ary Section of Roadbed on Embankment.



... Section of Roadbed on Embankment.



is obtained from a distance; by keeping the above principle in mind and utilizing surplus excavation to that end, the banks can be put in an enduring shape with an ultimate saving in cost.

F. S. Washburn, M. Am. Soc. C. E.—The title of the paper, as I understand it, is "Form of Railway Excavations and Embankments;" and in examining the diagrams the eye is first impressed with the differences in form rather than with any novel mode of construction. But closer inspection develops to my mind that, so far as the forms of the proposed sections are concerned, they are very nearly what Nature will herself make the ordinary slopes in the course of a short time. I cannot see that a paper, based simply upon the rounding off of the angle on top, and rounding the angle at the ditches, would have great practical bearing. The paper says very little of what the diagram shows, upon close inspection, to be very important. It indicates that the slope is to be sodded during construction; that ditches are to be paved, and tile-drain laid. The futility of a young assistant engineer's pride in his new earth slopes has been dwelt upon, but let us imagine for an instant the state of mind of a chief engineer, whose assistants asked for sod, broken stone and tile-drains on new construction. In short, it seems to me, that the importance of the form of the embankment, as bearing upon the cost of maintenance, is very small, and that the cost of maintenance is reduced to a much greater extent by the use of sod and paving and tile-drains, and that this feature of the subject is not dwelt upon to a very great extent in the paper.

K. E. Hilgard, M. Am. Soc. C. E.—The preceding speaker reminds me forcibly of the first "set-down" I was served with when engaged upon railroad construction through prosperous farming and suburban property in one of the Northwestern States.

I had accidentally commenced my practical engineering on railroad and canal construction in beautiful Switzerland, and there I had been used to seeing the embankments sodded over, or seeded at least, in due season after their completion. Of course, I was impressed with the fact that this afforded an excellent and cheap method of protection, if judiciously applied in its proper place. I was still young when I advanced the opinion to my then chief over here that the same should be done in that case; but I shall not easily forget his prompt, expressive answer.

I am glad to know that so eminent an engineer and authority as Mr. Whittemore should personally in so few lines state that there is even economy in this method, thus strongly advocated by him, and I feel vindicated now, and that I may forgive my former chief, and subsequent friend, forever.

Mendes Cohen, Past President Am. Soc. C. E.—In listening to this paper I am impressed with the fact that we must be coming to the millennium in railroad practice.

I am surprised and gratified to find that our honored Past President, the distinguished chief engineer of a prominent line of railway, is able, as the result of his long experience, to recommend to the younger members of the profession so thoroughly effective a treatment of the slopes of excavations and embankments.

The trouble always was, in years gone by, that with extremely limited means engineers were expected to secure a graded roadbed at the least cost, and that meant necessarily with the least possible amount of excavation or embankment, as the case might be.

Most of us were well aware that an increase of the angle of slope, as also thorough drainage, would save future labor and expense in maintenance, but how rarely was there a chance to put such knowledge into actual practice. A plea for it would not have availed much with our financial managers, who seldom had money in advance of present urgent requirements.

The slopes would wash and completely fill the ditches, which the contractor would be required to clear at the last moment before getting his final estimate. But how very soon it was after his departure that the process had to be repeated, and so on almost continuously.

If we had seriously suggested to our boards of managers the importance of giving ample width to the slopes, to be immediately followed by sodding them, we would have been laughed at; and had the proposition been tolerated and the work attempted, we would, in all probability, have very soon found ourselves out of employment, for the cost would have been overwhelming, and our railroad companies would have been bankrupted sooner than so many of them have been.

All that we could do in earth excavation was to round off the top of the slope, and if it crumbled off, to endeavor to prevent its falling into the ditches.

It was deemed good practice in such cases to make a berme or bench along the slope at about the height of a platform car, so that the berme should catch the greater part of the wash from the slopes and prevent its falling into the ditch, holding it at this higher level, whence, at convenient times, it could be more economically handled and loaded directly with the ditching train.

If, in the case of our older roads, we have now reached the time when the slopes having been widened by natural or special causes, we can with comparatively little labor dress them off and plant or sod them, we are coming to a very desirable condition, but to one that could not have been attained at the outset.

It seems to me that even now it would be a simple impossibility in this country to grade a new road and talk to the authorities about sodding or planting the slopes.

With regard to the matter of drains at the top of the slope in exca-

vation, my own experience does not follow that which one speaker referred to : that ditching at the top of the slope is always injurious ; I have found quite the contrary. Ditching at the top of the slope where there is much drainage into the cut I have found the only remedy from serious trouble. If the soil is of a character which is readily permeable and the water can come down through, we must construct the drain with paving or boxing in order to carry the water off and prevent its cutting in. The draining of the top of the slope I have always considered as very important; I never deem the work so poor but that we can afford that. Draining at the bottom is also very important.

D. J. Whittemore, Past President Am. Soc. C. E.—I partly agree, and perhaps may quite agree, with our Past President, Mr. Cohen, that it is not always policy to have surface ditches at the outside, and in some places it may be policy to have tiles. In the region in which I work there is hardly any instance where it would be policy to have those ditches outside and above the slope. The earth is easily saturated with water, and it is quite difficult to compel road repairers to attend to such ditches.

About railway companies not permitting the engineer to employ the devices I have mentioned in this little paper, I believe that, if the engineer would properly assert himself, in many instances the expenses of sodding these slopes and forming them would be saved during the first year of operation ; preach that to managers, and I believe they would act upon the advice. I think that is perhaps where our difficulty has arisen, that we have not gone to the president and the active directors of railways and talked as we should. I find no difficulty with the directors of my line in this very matter ; of course, they have had long experience, and as the manager of our line in one case remarked: "We must do that on our next work ; if we had put it on our last 200 miles of road we would have saved thousands of dollars." That is the fact of the line with which I am connected, and I am pretty confident that if the younger, and older members too, talk this up with the companies, they will succeed very often, and, at the same time, save nearly the entire expense of such work during the first year of operation of the road.

Desmond FitzGerald, M. Am. Soc. C. E.—It may gratify Mr. Whittemore to know that in Massachusetts we have built embankments as he suggests in this paper. I believe the ideas advanced are practical. Materials of which embankments are made differ, however, so widely, according to the section of the country, that general rules must be applied with judgment. In New England, in order to protect the sides of embankments, it is necessary to have a good depth of loam upon the gravelly materials forming them. Sodding alone without plenty of loam is soon lost. I have often seen embankments built

under specifications which called for 9 ins. of loam. I don't know where that 9 ins. originated, but I have seen it for years in specifications. Two or three feet of loam are necessary to form a permanent sod. It may seem extravagant, but in building high embankments I have always put on as much as 2 ft. where possible. If less is put on, the grass will not hold its own through droughts.

Mr. Whittemore.—While it has been about 45 years since I had my first division in New England, I recollect well the distinctive nature of the soil; it was sandy gravel, and it would be quite an undertaking on that line of road to maintain seeding. It could be done, of course, but there was not that necessity for it in a soil of that dry nature as there is in the vast regions of the West, where there is a clay of loam, and, I venture to say, that in 6 000 miles of road I have in mind there are not over 100 or 200 miles where you would have to do very much to produce soil on slopes of cuts that would maintain the growing of grass. If such places as that are found, I should suggest always sodding. Sod is easily to be procured, and by proper staking I have found very little or no difficulty in its continued growth. There is a distinction between the various portions of our country; what will work in one locality will not in another. In the large regions of the West, and in the prairie country, I can see no difficulty in maintaining sod upon the surface of the slopes.

Benjamin Reece, M. Am. Soc. C. E.—In regard to rounding off the top edges of embankments as here advocated, I will say in the work of cutting down grades on the Lake Shore and Michigan Southern Railway, in my charge from 1881 to 1887, the advantage and economy of the proposed change was strikingly illustrated. In lowering many of the cuts the work was done by steam shovels, and where the position of curves would permit, the original track was left undisturbed to carry the traffic, while the new roadbed was excavated close by the side of the old one to the proper depth required for the new grade.

To avoid delays which the purchase or condemnation of additional right of way would entail, these excavations were kept as close to the original track as the safe carrying of trains thereon would permit.

These improvements were of such magnitude as to frequently consume several months' time before their completion, and in the earlier days of the work much trouble and considerable expense was experienced by the top edges of the cuttings breaking away and sloughing off from under the ends of the ties of the track. Night and day watchmen patrolled the track in advance of the trains and the necessary repairs were kept up. As soon as this tendency became apparent the workmen were required to round off the exposed top edge of the roadbed, and from that time on no further trouble was experienced. The advantages thus derived under extreme conditions described very

clearly points to the scientific basis and practical value of the suggestions made by the author of the paper.

The form proposed is the one which the trackman must ultimately give the slopes of his embankments and excavations in order to make them stable. If, as has been suggested, the slopes would of themselves assume the form proposed, there would be little trouble, but they do not, for usually the earth sloughs off in very considerable masses, filling up the ditches of the cuts and reducing the top width of embankments, calling for constant labor at considerable cost to clean out the one and to reinforce the other.

There is little doubt in my mind that for every yard of earth handled at small cost in construction to perfect the form of excavations and embankments as indicated by the author, the trackmen will be saved the necessity of handling many yards at high cost in the work of repairs.

Railways are built to be operated, and the engineer builds best who has due regard to the necessities of maintenance as well as of traffic.

By preventing the first tendency of the earth to move, which in ordinary soils is by no means difficult, much money and labor can be saved.

As further illustrating this question, I will say that in raising the embankments for the changes of grade and second track, it was found that in the higher fills with practically the same material the quantities of earth required for a given raise showed quite a variation with no apparent cause. A study of this feature soon revealed the fact that where the ends of the slopes terminated in the old ditches, the movement out was prevented, and the earth of the slopes was compacted upon itself, affording in the aggregate a considerable saving in amount of material moved, as well as giving a better consolidated and more stable embankment. After this observation a small footing trench was dug along the marginal edges of the foot of embankments and the little extra labor of so doing was repaid many fold.

In preparing the site for the new passenger station of the Lake Shore and Michigan Southern Railroad at Toledo in 1886, it was necessary to widen the excavation at that point, which was from 20 to 25 ft. in depth. The first 6 or 7 ft. from the surface was a friable yellow clay, the rest of the depth was through a tough blue clay which was rendered more or less unstable by the presence of sand pockets and seams.

The original cut, notwithstanding the free use of drain tile, had always given more or less trouble and at one time necessitated the removal of a slide containing some 200 carloads of earth. The work of excavation was done by steam shovels which left the sides almost perpendicular. As the top slope when formed constituted one line of a street running parallel with the railroad, its stability was

a question of prime importance. In shaping this slope a small V-shaped trench about 1 ft. in depth was cut along where the foot of the slope would fall, the perpendicular walls of the cut as left by the steam shovel were broken down, forming the slope, which was allowed to settle and the small V trench holding the toe of the slope from pushing out, it soon consolidated, when drains of cobble stones were laid obliquely down the face of the slope, which was then seeded to grass, and up to the present time no further trouble has been experienced. When tile drains were used a slight movement of the face of the slope would tend to destroy the connections of the tile drains, leaving the water to percolate through the cut, which caused more or less frequent slides, but with a good body of cobble stone forming the drains such movements of the slope caused a slight attenuation, but did not destroy the continuity of the drains and the conditions of good drainage were preserved.

The character of the materials handled has, of course, much to do with the methods which should be employed; nevertheless, as a general proposition, the methods advised in the paper can be defended and sustained as being more efficient and less costly than those which now prevail.

J. M. Knap, M. Am. Soc. C. E.—In the matter of sodding, if the course pursued by the Ogdensburg and Lake Champlain Railroad Company in the construction of their road were generally adopted, it would be a good thing. This company had incorporated in its contracts that the slopes of cuts should be sodded, and withheld part payment until it was done.

One piece of their work (a cut some 20 ft. in depth and about one mile in length), done 40 years ago, was sodded at the time, and is the admiration of all who pass over the road; the slopes are in good shape to-day, and have been maintained at little or no expense. The extra cost to the company at the start was trifling, although it must be admitted that the contractor did fail.

Mr. Crowell.—The value of such luxuries as sodded banks is manifested on all roads that can afford to make a choice between the two methods. The practice has generally been settled by the crude test of first cost, but the tendency now is toward the crucial test of maintenance charges. I quite agree with the proposition stated, that many roads could not be built if this expense were added to construction account; there is not money enough in hand in building many new roads to go to such expense; but that has nothing to do with the best practice. Makeshifts are often necessary in new construction; but if there is money enough in hand to secure it, the permanent character secured for embankments and cuts at the earliest possible moment is an element of economy in the operation of the road. The sodding of a bank, which looks like an ornament in a well-kept cut, is not a luxury at all, but a money-saving contrivance.

In regard to the question of ditches at the tops of slopes, my proposition was misunderstood; I should be the last to say that the ditch was not necessary there. What I did say was that the presence of the ditch on the top of the slope was apt to produce a saturation of the ground, making it all the more desirable to flatten the top of the cut. It is impossible to avoid such ditches, but still they are often used to too great an extent. Water is not infrequently carried along them which ought to be taken down to the track level by the shortest path, or be otherwise disposed of, so as to get it away from the slopes. Slope drains, with paved channels which carry the water directly down the slope, are often used with good results. Established railroads, when extending or widening out, increasing the number of tracks, resort to sodding at the earliest possible moment, chiefly to reduce the maintenance charges, and that should be the object in every case; although it is, of course, impossible to secure, in the first year of a railroad embankment, the permanence which will come in time.

J. F. Wallace, M. Am. Soc. C. E. (by letter).—If the ideas advanced by Mr. Whittemore had been put into practice years ago, the result would have been a saving of hundreds of thousands of dollars to railroads in the central and western portions of the United States. The past and present practice of making the cross-section of railroad excavation and embankment conform to the theoretical lines laid down by the earlier engineers has so far given expensive and unsatisfactory results.

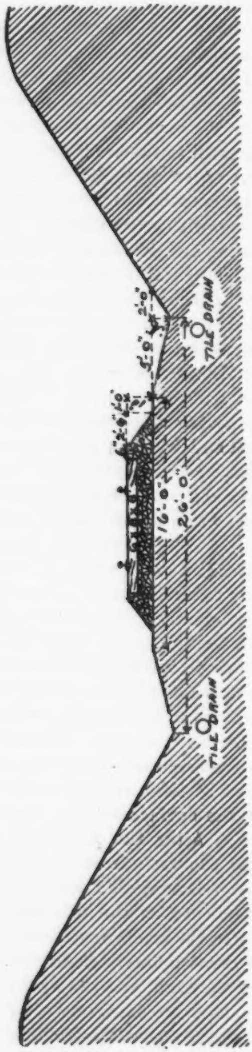
The writer has known cases where it has required as great an expenditure of money to obtain and maintain a good roadbed through excavations as it originally cost to make the excavations, due to the sloughing-in of the sides of the excavations, the filling up of the ditches, and the imperfect drainage caused thereby; necessitating, not only the expensive method of cleaning out surface ditches by the use of work-train service, but also a waste of money in keeping the track in proper condition while the work was under progress.

Most of the railroads constructed in the south and west have rarely been fully ballasted at the time the work was originally done, the ties being laid on the surface of the ground, and surfaced slightly with ordinary soil. After the first few rains the original ditches would be filled with the wash from the slopes or the sloughing in of the sides of the cuts. During the wet season it would be almost impossible to ditch the cuts, as the material in the ditches would be a soft slush; frequently the track would be blocked up by using extra ties at a cost of 50 cents apiece. Sometimes the tracks would disappear entirely below the mud, and the writer knows of one case in which two additional tracks were laid, one after the other, on top of the original track, in order to maintain traffic.

The form of cross-section suggested by Mr. Whittemore would

I.C.R.R.

STANDARD ROAD BED IN CUT



SECTION OF STONE OR COARSE AND LOOSE NON CEMENTING GRAVEL BALLAST IN CUT

largely prevent this, but not entirely so of itself. This form of cross-section used, however, in connection with drain tile as suggested would give good results. The systematic use of drain tiles in earth excavations is the best remedy for the sloughing in of the sides of the cuts and the filling up of the surface ditches.

There are a few roads, however, in the central and western portions of the United States where drain tile has been systematically used. In 1892 the writer commenced on the Illinois Central Railroad the systematic use of drain tile in excavations, and with the most satisfactory results, and it is the intention to pursue the use of tile until all the cuts that are wet or springy have been thoroughly tiled. The minimum size of tile used is 6 ins., and the maximum 12 ins. Several long cuts that were tiled in 1892, where the grade of the track was one-half of 1%, and the cuts from 3 500 to 4 500 ft. in length, the 12-in. tile was used for a few hundred feet from the mouth of the cut, and was then reduced as the work proceeded toward the summit from 12 to 10 and 8 ins., and then to 6 ins. in diameter.

The cross-section suggested by Mr. Whittemore will be of little avail unless sodding is done at the time the work is constructed; and the writer would also suggest a modification of his cross-section as per the diagram on page 264.

As an illustration of the expense entailed by not taking proper means to protect the slopes and provide for subdrainage during the period of construction, the writer would refer to one division of road that was constructed five years ago. Upon 50 miles of one section of this line, the writer now has two ditching trains which will be engaged for five months in cleaning out the surface ditches, which, in his opinion, would have been avoided if the work of construction had been carried out on the lines suggested in Mr. Whittemore's paper.

Since reading Mr. Whittemore's paper, the writer has adopted the cross-section shown on diagram as the standard cross-section for new excavations on the Illinois Central Railroad. A cross-section of similar form, but with a slope of 2 to 1, has been used by the Illinois Central Railroad Company between Dubuque and Sioux City, and on the branches in Iowa. The use of this cross-section was due to the existence of a large number of shallow cuts that only needed surface ditching; it was also desirable to have a curved form of cross-section and flat slope in order to prevent the accumulation of snow. The old excavations were worked over with ordinary drag scrapers. This work was done at an approximate cost of 10 cents per cubic yard, and the waste material spread uniformly over the right of way outside of the excavation. This was not only an economical way of ditching the excavations, but the using of the flat slope and the curved cross-section gave the wind a chance to sweep the excavations clear of snow, and prevented the deposit of drifts. This plan was

adopted two years ago, and has given satisfactory results, both in the ditching of the cuts and in preventing deposits of snow during the winter season.

Charles H. Snow, Assoc. M. Am. Soc. C. E. (by letter).—A gentleman has spoken of the advisability of placing a layer of top soil on the slope of the otherwise composed railway embankments. It is, of course, obvious that if embankments are to be protected by vegetation, turf or seed must not alone be applied but be kept alive, and they can alone be kept alive by congenial soil, such as banks are not always composed of. There is, then, the difficulty, not alone of procuring turf or seed, but frequently top soil as well, and it would seem that if objections were raised to the cost of sod or seed, the same would be much magnified in the matter of top soil, as necessary as the others. Sufficient top soil would often form a large proportion of embankments, so that in some parts of the country the embankments would be largely composed of material which must be brought from 5 to 500 miles. Again, the wide strip of our Western Continent, crossed by all of the overland routes in which vegetation is not possible without irrigation, must be encountered. If the suggestion of the author of this paper were to be applied generally, instead of locally, as he intended, some provision would be needed for irrigating such tracts.

Those of us who visited Sandy Hook last January noticed some places in which sod did not entirely cover embankments, but squares of it were placed alternately so that the whole appeared like a checker board.

Mr. Whittemore (by letter).—Further remarks of the writer will be as brief as the paper itself.

Of course there are localities where, during initial construction, it will not be reasonably practicable to secure and maintain a growth of sod along slopes, particularly where the line traverses regions destitute of soil, and where embankments and excavations are of clean gravel and sand, and it is equally true that in such localities generally little trouble is experienced in securing stability of slopes as affected by water. The writer claims that his suggestions apply with marked force to those extensive regions through which lines are built, having soils more or less rich, and which are in many instances several feet in depth, and in which the unprotected slopes become mud producers during and after storms. The necessary width of excavations at formation level depends in degree upon the depth and kind of ballast used. The writer's sketch indicates a section found serviceable where fine gravel is used; when coarse gravel, broken stone, or calcined gumbo is used, a different form of section is advised.

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731.
(Vol. XXXII.—September, 1894.)

THE TEQUIXQUIAC TUNNEL.—DISCUSSION ON PAPER No. 725*.

By DON LUIS ESPINOZA.

Ever since Cortes established the present city of Mexico on the site of the ancient Aztec Venice the problem of the drainage of the valley of Mexico may be said to have occupied the attention of its inhabitants. The projects for dealing with the difficulty have been numerous, and very large amounts of money have been spent and much work done since the time referred to; work which in some cases has been partially successful, or which, at any rate, has tended (aided by the changes in the rainfall of the district, contraction of the area of the lakes, etc.) to modify considerably the old conditions and to simplify somewhat the problem.

Among the first projects which were advanced at the commencement of the 17th century, was that of engineer Henry Martinez, which, having been approved and started, served to commence the work known as the Virreyes; this was at the time when there was peace in Mexico under the dominion of Spain. The plans of Martinez were

*"Tequixquiac Tunnel, Valley of Mexico." By Albert Johnstone Campbell and Frederick William Abbot, Assoc. M. Am. Soc. C. E. *Transactions*, Vol. XXXII, page 171.

entirely different from those which are now being carried out, and it is not necessary to follow the different branches of said plans, but only to say this much : that they consisted at first in plans for a tunnel discharging in the neighborhood of the town of Huehuetoca, for which, on account of the falling in of the roof, an open cut, which is now known as the cut of Nochistongo, was substituted. This work only served to deviate from the narrow enclosed basin called the Valley of Mexico a part of the water which the rains precipitated into it.

Thus this tunnel, on account of its having been started at a level higher than the bottom of the valley, did not solve the problem of drainage.

The plan which is now being carried on, and known as the Tunnel of Tequixquiac, is a complete solution of this difficulty, not only drawing away from the valley the waters which run into it and thus flow into the city of Mexico, and avoiding the constant danger of inundation to which it has always been exposed, but at the same time arranging a plan by which it will be possible to drain the city thoroughly, giving a sufficient flow to the sewers which will be constructed to perfect its sanitary drainage.

The plans for the Tunnel of Tequixquiac were also suggested in the 17th century by Simon Mendez, a miner, and were approved and started after the tunnel built by Martinez had fallen in, but were soon abandoned on account of excessive cost. Later, in 1774, the plan of the Tunnel of Tequixquiac was carefully studied by the learned Don John Velazquez of Leon. The problem was again studied by Lieutenant Smith, of the Army of the United States, in 1848, who was authorized by the Drainage Board of Mexico to make such investigations ; and, by order of the meeting of the Drainage Board of the city of Mexico in 1855, was again studied by Engineer Don Francisco Garay. Still later, during the empire of Maximilian, the present project was taken up by a special commission of engineers, and the work started. Several of the shafts on the line of the tunnel were excavated to a considerable depth, and at the same time work was commenced on the construction of offices, etc., and the erection of machinery. The fall of the empire paralyzed the work for some months, but the Government of the Republic commenced it again, going over the plans once more, ratifying them, and deciding that the present line of the tunnel was the most convenient.

After having finished, under the administration of President Señor Jaurez, in the years 1868–71, a large amount of work, such as deepening the shafts and installing machinery, as well as the construction of the open cut at the outlet of the tunnel, about 2 000 m. long, with a maximum depth of 27 m., the work was stopped on account of the general disturbance in public matters, and was not again started until 1885, when a special commission or drainage board was formed by General Diaz, and work under the direction of the writer, who was the government engineer, was again commenced. From 1871 to 1885 all that was done was to preserve the work which had been completed, and to take care of material, etc.

In the opinion of various engineers the different projects which have been designed for this work show a tendency towards too great size in the dimensions of the cross-section of the tunnel ; at the same time it was peculiar that in all of these plans there were no two alike, which necessitated a new calculation each time. This was finally made by the writer, who determined the quantity of water which it was necessary to handle for the drainage of the valley of Mexico, which quantity he places at 17½ cu. m. per second, and also determined the dimensions necessary to make the tunnel sufficiently large to handle this quantity; and he it was who ultimately designed the section which is now being built.

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732.

(Vol. XXXII.—September, 1894.)

FRICTION ROLLERS.—DISCUSSION ON PAPER No. 722.*

By J. B. JOHNSON, M. Am. Soc. C. E., and A. MARSTON, Assoc. M. Am. Soc. C. E.

J. B. JOHNSON, M. Am. Soc. C. E. (by letter).—This is the most valuable contribution to our knowledge of the strength, friction and wear of metallic rollers that has come under my observation. Professor Marston has rendered us a good service in his elaborate analysis of the distribution of stress in a metallic roller, but I would prefer to see the development of the subject based on the distributed stress which obtains in practice rather than upon a concentration of stress on a linear element of the roller, as has been assumed in the paper. I see no advantage, for instance, in the development of formula (15), giving the area of contact on the assumption that the load is wholly concentrated on a mathematical line. This method of concentrating the load would produce an indented roller and plate, with an infinite intensity of stress along the loaded line. The assumptions involved in equation (15) are, therefore, purely hypothetical and far fetched, being not at all applicable to

* "Friction Rollers." By C. L. Crandall, M. Am. Soc. C. E., and A. Marston, Assoc. M. Am. Soc. C. E. *Transactions*, Vol. XXXII, page 99.

the determination of the area of contact, and might therefore be omitted from the discussion. Immediately following this development, however, the author gives the result of a development of the theoretical area of contact when the true distribution of stress is used. The mathematical analysis involved in this result might well have been inserted in the paper in place of the conventional assumption involved in formula (15). A rational analysis for determining this theoretical area of contact has never yet been published, so far as I am aware.

It is also unfortunate that no record has been given of the areas of contact corresponding to the experiments plotted on Plate XVIII. I am inclined to give great weight to these experiments, as they seem to have been made with adequate care and with intelligence. If the author of the paper could give us the law of the areas for increasing loads when the diameter remains constant, and also the law of the areas for constant loads with varying diameters, he would add largely to the value of his paper, so far as the theoretical analysis of the problem is concerned. He has, however, given us no information as to these areas, and perhaps they were not taken.

So far as a working formula is concerned, however, for the strength of a metallic roller, the results plotted on Plate XVIII seem to be entirely satisfactory. They show conclusively that the crippling load on a metallic roller varies directly with its diameter. This conclusion is quite independent of any theory of stress distribution, and would seem to warrant the use of a working formula like that given in equation (17), in which the working load per unit of length of roller is some constant multiplied by the diameter of the roller. So far as practical use is concerned, this is the main question, and the paper would seem to be conclusive on this subject.

I will take the liberty of adding here some results of tests for determining the area of contact between locomotive and car wheels and rails obtained by me five years ago, when the subject of "Wheels and Rails" was so prominently before this Society. A section of a steel-rimmed locomotive driver 44 ins. diameter and a section of a 33-in. diameter cast-iron car wheel were procured and mounted in a 100 000-lb. Riehlé testing machine. Short sections of steel and iron rails were also procured and placed in the machine, so that the wheel-treads rested upon them in a normal position. They were then loaded with 5 000-lb. increments, from 5 000 to 60 000 lbs., the area of contact

being measured after each loading. These actual areas of contact are given in Plates XXXIX, XL and XLI, ~~where they are shown to a scale one-half the actual size, thus making the areas one-fourth actual size.~~ In Plate XLII these areas are plotted for both the 33-in. and the 44-in. wheel.

The surfaces of the wheel-treads and also of the tops of the rails were highly polished, so as to become good mirrors, before the experiments were made.

The areas were obtained by marking with wet chalk the surface of the rail and rubbing it down with a moistened finger and allowing it to dry. This gave a whitish appearance to the surface, with no appreciable thickness of film, and the actual area of pressure contact was clearly shown by a slight discoloration of this surface. A pencil line was passed around the border of this contact area, and then the area copied on tracing paper. The surface was then cleansed and examined for permanent distortion, and prepared for another loading.

No permanent distortion was noted upon either rails or wheels at the contact surface up to the 60 000-lb. limit. At this loading, however, the web of the rail was observed to take a permanent set.

The most remarkable feature of these tests would seem to be the law of increase of area with increasing loads, as shown in Plate XLII. These areas plot practically upon a straight line through the origin, indicating that the area is directly proportional to the load. This being true, it must follow that the load divided by the area of contact, or the average stress per square inch over the area of contact, is a constant for all loads. This constant is something over 80 000 lbs. per square inch, but the distortion or compression of the surface at the center of these areas of contact is just twice the average distortion over the area, and, since the loads seem to be within the elastic limit of the material, it follows that the intensity of stress at the centers of these areas of contact were twice the average intensity of some 80 000 lbs. for the steel driver upon a steel rail, or that the maximum intensity of stress at the centers of these areas was for steel upon steel 160 000 lbs. per square inch. For the chilled cast-iron wheel 33 ins. in diameter this maximum intensity was still greater as the area was less.

These areas were obtained by placing two cylinders in contact, their axes being at right angles. The volumetric distortion for each

IMPRESSIONS ON WORN IRON RAIL. TOP RADIUS, 11 INS. FULL SIZE.

Direction along the Rail.

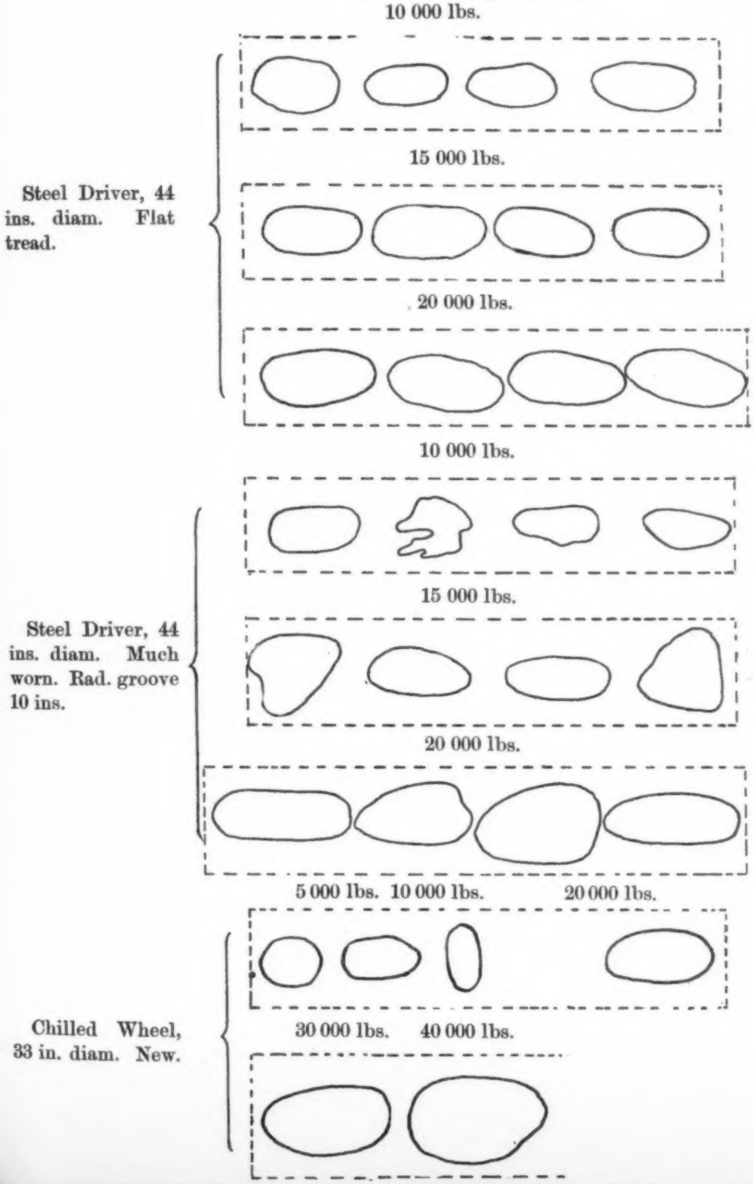
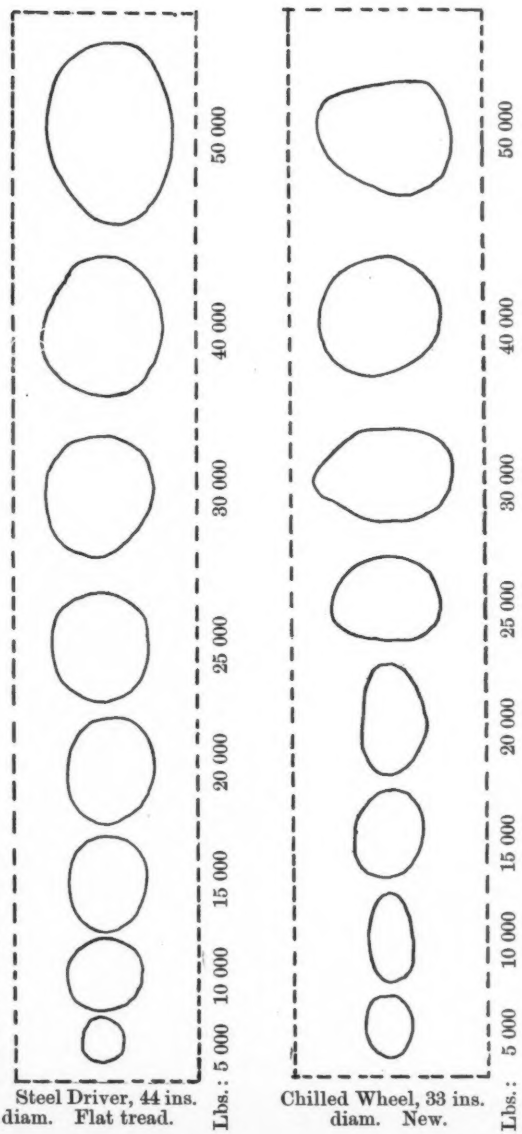


PLATE XL.
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXXII, No. 732.
JOHNSON ON FRICTION ROLLERS.

IMPRESSIONS ON 52-LB. STEEL RAIL. TOP RADIUS, $13\frac{1}{2}$ INS. FULL SIZE.

Direction along the Rail.



Steel Driver, 44 ins. diam. Flat tread.

Chilled Wheel, 33 ins. diam. New.

PLATE XLI.
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXXII, No. 732.
JOHNSON ON FRICTION ROLLERS.

IMPRESSIONS ON 75-LB. STEEL RAIL. TOP RADIUS, 14 INS. FULL SIZE.

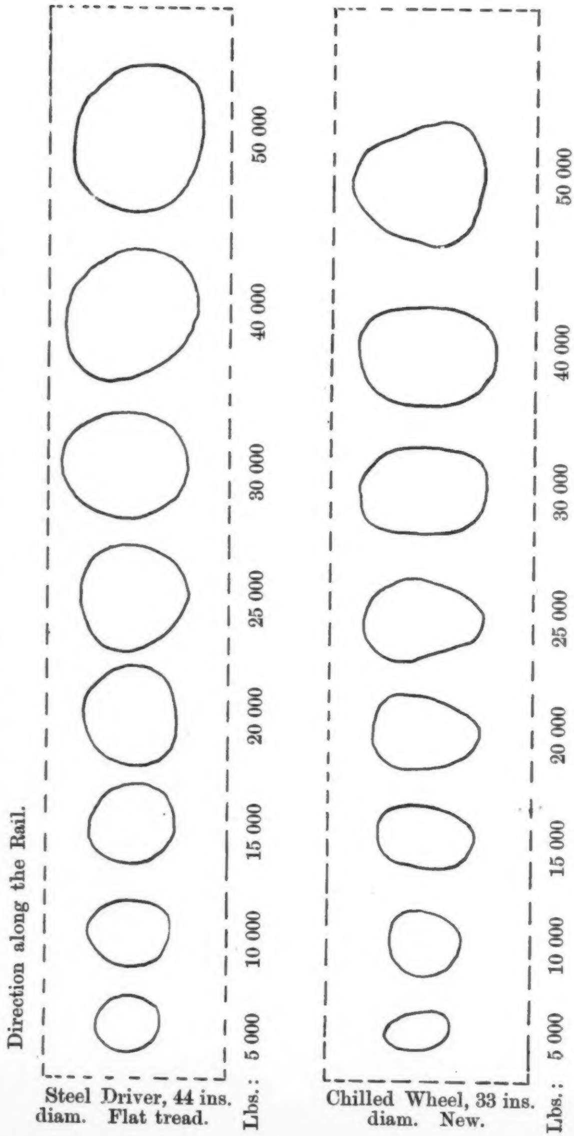
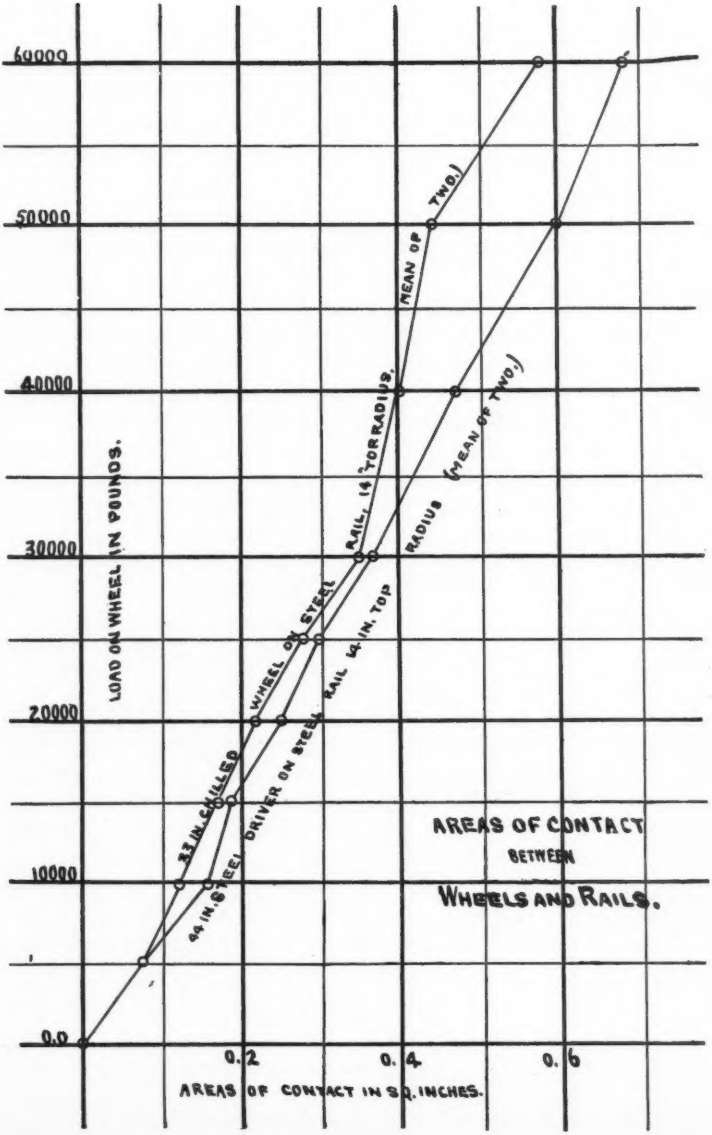


PLATE XLII.
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXXII, No. 732.
JOHNSON ON FRICTION ROLLERS.



of the surfaces in this case is fairly represented by the segment of a paraboloid. A property of the segment of a paraboloid is that the maximum ordinate is twice the mean ordinate, or, in other words, the volume of the segment is one-half the enclosing cylinder. This fact authorizes the statement made above, that the maximum distortion at the centers of these areas of contact is twice the average distortion, and hence the maximum stress is twice the average intensity of stress as above stated.

I have withheld these observations from publication, thinking I might be able to find a rational analysis of the problem explaining the distribution of stress and this law of area of contact. If Professor Marston will develop the analysis leading to his conclusion concerning the area of contact for a cylinder upon a plane for distributed stress as given by him on page 122, following equation (15), and will then adapt the analysis to two cylinders in contact, he will have furnished the requisite theoretical explanation of the areas given in Plates XXXIX, XL and XLI, and may be able to explain the law of variation of area of contact with load, as shown in Plate XLII. A simpler case to work out would be that of a sphere upon a plane, and the analysis of this case would doubtless be very nearly applicable to the case of two cylinders crossing each other. The analysis of these two cases, namely, a cylinder upon a plane, and a sphere upon a plane, considering the stress distributed over the areas of contact according to the laws of the parabola and of the paraboloid, with due allowance for the distribution of the stress through the body of the roller, according to the laws already determined by Professor Marston, would, I believe, show a close agreement between the computed and the actual areas of contact and would serve as the last word on this subject from the mathematical point of view. As the paper stands, however, it contains no such theoretical analysis. I venture to hope that Professor Marston will supplement his paper with the true analysis of this problem.

A. Marston, Assoc. M. Am. Soc. C. E.—A copy of the paper in the "Physical Review," referred to by Mr. Hutton,* was sent to the Society some months since.

The formula quoted by Mr. Hutton from Résal is the particular form of the Grashof formula referred to in this paper at the commencement of the discussion of formula (14). The assumption that the value

* *Transactions*, Vol. XXXII, page 127.

of the maximum pressure at the elastic limit is 30 000 lbs. per square inch for steel on steel may be compared with Professor Johnson's conclusion, arrived at by actual measurement of the areas of contact, that the maximum pressure in the case he cites was 160 000 lbs. per square inch.

With regard to the assumption made in this paper that the stresses and displacements in plates and rollers are, except very near the area of contact, the same as if the loads were applied along a line instead of over an area of contact, it may be said that this assumption is entirely similar to the one always made in the common theory of beams, that the stresses are the same as if the concentrated loads were applied at mathematical *points*. The correctness of this assumption was enunciated by Saint Venant, has been proved by Boussinesq, and may be considered as well established.

The writer is satisfied that, owing to the way in which the load is distributed over the area of contact, the forms of the surfaces of the roller and plate under strain will be so nearly the same as indicated by equations (5) and (11) up to the very edge of the area of contact, that the use made of the above principle in deriving a correct *form* for the empirical equation (15) for the area of contact is perfectly legitimate. This conclusion was strengthened by the fact that Professor Johnson has derived this equation empirically from the results of the experiments of Professors Crandall and Wing and that his values for the constant K are nearly *proportional* to the $\sqrt{\frac{c_r + c_p}{\pi}}$ of equation (15).* Moreover the writer checked this assumption by computing the value of x for which $\frac{dy}{dx}$ for the plate equals $\frac{dy}{dx}$ for the roller (*i. e.*, at the edge of the area of contact), for the common assumption that the stress is distributed over the area of contact according to the law of the parabola. The result of this analysis was given in the paper, but the analysis itself omitted, because complicated, and because we have no knowledge that this distribution of the stress is the true one. The analysis, however, is presented here, at Professor Johnson's request, as follows:

The assumed distribution of stress is $G = \frac{3}{4}\frac{W}{a}\left(1 - \frac{x^2}{a^2}\right)$, where G is the stress at any point of the area of contact, and the other sym-

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* See "Framed Structures," page 277.

bols are the same as those used in deriving equation (15). From equations (5) and (11) it can be shown that the *variable* part of the vertical displacement at a point x_1, on the surface of either plate or roller, caused by the forces $G\,dx$ applied at $+x$ and $-x$, is $d\,v = -\frac{c}{\pi} G\,dx \log. (x_1^2 - x^2)^2$, where $c = \frac{(1-\sigma)(1+\sigma)}{E}$. Hence the total displacement—

$$v = -\frac{3\,c\,W}{2\,\pi a}\int_o^a \left(1 - \frac{x^2}{a^2}\right) \log. (x_1^2 - x^2)\,dx, = -\frac{3\,c\,W}{2\,\pi a}$$

$$\left[x \log. (x_1^2 - x^2) - 2\,x + x_1 \log. \frac{x_1 + x}{x_1 - x} - \frac{x^3}{3a^2} \log. (x_1^2 - x^2) + \frac{2\,x^3}{9\,a^2} + \frac{2\,x x_1^2}{3\,a^2} - \frac{x_1^3}{3\,a^2} \log. \frac{x_1 + x}{x_1 - x}\right]_o^a$$

$$= -\frac{3\,c\,W}{2\pi\,a^3}\Big[(2\,a^3 + 3a^2\,x_1 - x_1^3) \log. (x_1 + a) + (2a^3 - 3a^2\,x_1 + x_1^3) \log. (x_1 - a) + 2a\,x_1^2 - \frac{16}{3}a^3.\Big]$$

Dropping the subscripts we may compute—

$$\frac{dv}{dx} = -\frac{c\,W}{2\,\pi\,a^3}\left[3\,(a^2 - x^2) \log. \frac{x+a}{x-a} + 6ax\right].$$

At the edge of the area of contact we have $x = a$, and the above becomes—

$$\frac{dv}{dx} = -\frac{3\,c\,W}{\pi\,a}.$$

Now $\frac{dv}{dx} = \frac{dy}{dx}$ for the plate, but for the roller we must subtract it from $-\frac{2x}{d}$ (the surface of the roller before strain being $y = -\frac{x^2}{d}$). Hence at the edge of the area of contact, where $\frac{dy}{dx}$ for the plate $= \frac{dy}{dx}$ for the roller, we shall have—

$$-\frac{3\,c_p\,W}{\pi\,a} = \frac{3c_r\,W}{\pi\,a} - \frac{2a}{d}, \text{ whence}$$

$$a = \sqrt{\frac{3}{2}} \times \sqrt{\frac{c_r + c_p}{\pi}\,Wd}.$$

(Note that a is one-half the width of the area of contact.)

The value indicated by this analysis for the constant K of equation (15) would be .000366 for the writer's experiments, assuming that $\sigma = \frac{1}{4}$, and remembering that E for this case is by measurement

27 300 000 lbs. per square inch. Prof. Johnson concluded from an examination of Crandall's and Wing's experiments that K should be .00055; but those experiments probably gave too large results, because at what the eye called the edges of the area of contact, the tallow films must have had some appreciable thickness. The following observations, just made by the writer with the 16-in. roller used in these experiments, indicate that the theoretical value of K given above is approximately correct.

OBSERVED AND THEORETICAL VALUES OF AREA OF CONTACT BETWEEN 16-IN. ROLLER AND PLATE.

Load per lineal inch of roller. Pounds.	Width of area over which light could not be seen between roller and plate. Inches.	Corrected width of area of contact. Inches.	Theoretical width of area of contact. $A = .000366 \sqrt{Wd}$. Inches.
0 +	0.05	0.00	0.000
1 000	0.10	0.05	0.046
2 000	0.12	0.07	0.065
3 000	0.14	0.09	0.080
4 000	0.15	0.10	0.093
5 000	0.15	0.10	0.103
6 000	0 16	0.11	0.111

The numbers in the third column of the above table were derived by subtracting from those in the second column the area over which the light could not be seen when the pressure was zero.

Two observations for the 10-in. roller gave similar results.

In conclusion the writer desires to call attention to two points in connection with the formulas developed in this paper.

The first is that all the stresses at corresponding points in different rollers are inversely proportional to the diameters of the rollers. This may readily be proved by substituting in equation (11) the values $x = a\,r$, and $y = b\,r$ (a and b being constants independent of r) which x and y would have at such points. This fact confirms the conclusion that the safe loads on rollers are directly proportional to their diameters.

The second point is that the value of Q_p in equation (6) furnishes a means for determining what thickness of bed plate under the friction rollers at the end of a bridge will meet with sufficient exactness the common requirement of bridge specifications that "the bed plates shall be thick enough to distribute the pressure uniformly over the

masonry." If W be the load per unit of length on the roller, t the thickness of the plate, x the perpendicular horizontal distance from any point on the lower surface of a bed plate to a vertical plane passed through the axis of the roller, and Q_m be the normal pressure on the masonry at that point caused by the roller in question, then—

$$Q_m = \frac{2\ W}{\pi} \frac{t^3}{(x^2 + t^2)^2} \quad \text{.................... (19)}$$

The total pressure on the masonry at the point in question can be found by adding the values of Q_m for the separate rollers, but usually only the nearest one need be considered. This maximum pressure should not exceed a safe bearing value, which, however, may be larger than the one specified when it is assumed that the pressure is uniformly distributed. It might be specified that the maximum pressure should not exceed the average pressure by more than a certain per cent. Computations on this basis will be found to encourage the use of thick bed plates.

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(Vol. XXXII.—September, 1894.)

HOISTING APPARATUS OF THE CANAL HEAD-GATES AT SEWALL'S FALLS, N. H.

By JOHN R. FREEMAN, M. Am. Soc. C. E.

READ AT THE ANNUAL CONVENTION, JUNE, 1894.

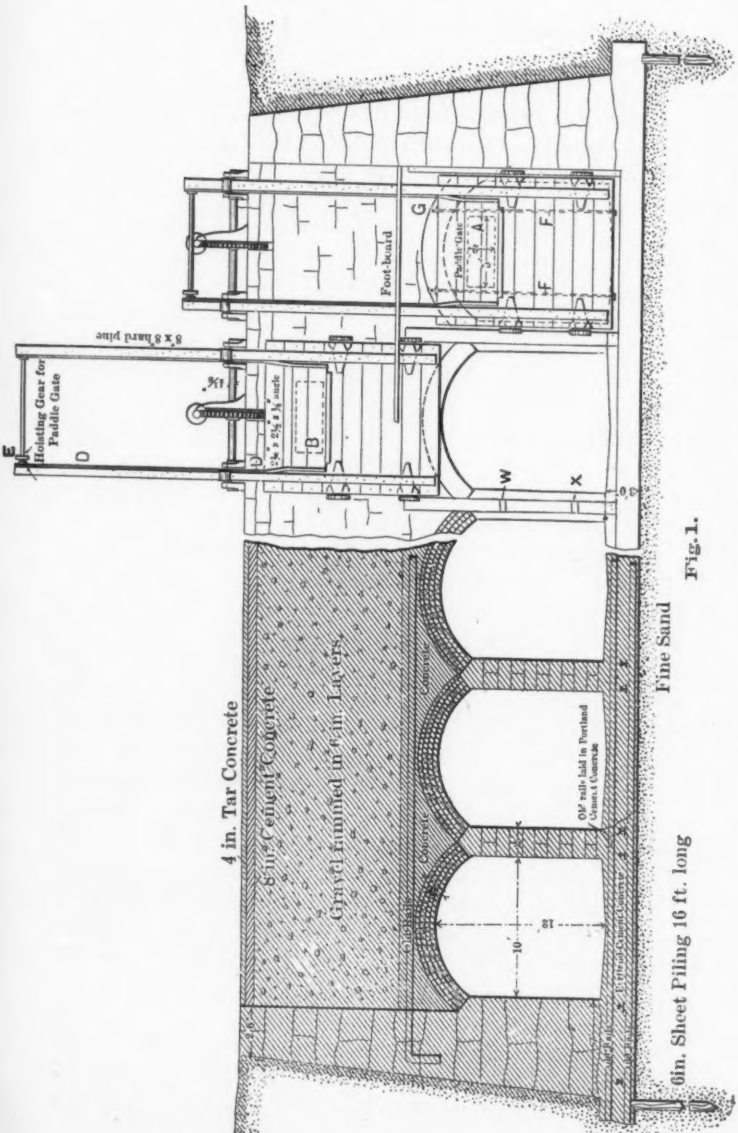
WITH DISCUSSION.

These head-gates are of interest from their large size and from the comparative ease and rapidity with which they can be raised or lowered by the hand-power of one man.

They were built between one and two years ago from the designs of the writer, and have but very recently been brought into practical use, and, so far as yet tested, appear to meet the conditions with success.

Although these sluiceways as a whole are but a modest structure, not so extensive or so elaborate in finish as many others, the arrangement for lessening the friction of each gate is novel, and the gates are the largest similar gates in a single leaf with which the writer is acquainted.

They are situated at the head of a water-power canal recently constructed at Sewall's Falls, on the Merrimack River, four miles above the city of Concord, in New Hampshire.



4 in. Tar Concrete
8 in. Cement Concrete
Gravel rammed in 6 in. Layers
Concrete
Concrete
Old rails laid in Portland Cement Concrete
Fine Sand
6in. Sheet Piling 16 ft. long
Hoisting Gear for Paddle Gate
8 x 8 hard pine
Foot-board
Paddle Gate

Fig. 1.

They adjoin a timber crib-dam, 500 ft. long, 16 ft. high, erected from designs of Edwin F. Smith, M. Am. Soc. C. E., which is an unusually interesting example of the very best grade of rock-filled timber crib-dam on gravel bottom.

It is designed that ultimately the whole ordinary flow of the Merrimack River shall pass through these gates to furnish power on mill sites lying between the canal and the river.

The natural flow of the river at this point, reinforced by the ordinary draft from the great storage reservoirs of Winnipiseogee, amounts during the dry season of the year to about 1 000 cu. ft. per second for the 24 hours. There will ultimately be considerable night storage in the pond formed by the dam, and at the hours of maximum work, or when water is plenty and the head is reduced by back water, the draft through the gates may, perhaps, sometimes be 2 000 or even 3 000 cu. ft. per second.

The floor of the sluiceways is 10 ft. below the present crest level of the permanent dam, and the top of the archway is level with the crest line of the present flashboards.

The permanent dam may, at some future time, be increased 1 or 2 ft. in height, and possibly 3 ft. more may be added by flashboards. The greatest freshet known along the Merrimack River for the past century would flow 10 ft. deep over the crest of the permanent dam.

The canal walls are carried to a greater height above the ordinary water-level of the pond than is customary. This was done for the purpose of allowing water to be carried high in the canal during freshets, and thus offsetting to some extent the loss of power due to back water. This arrangement was suggested by H. F. Mills, C. E., under whose supervision the original surveys for the development of this power were made about twelve years ago.

Figs. 1 and 2 show the general arrangement of the sluiceways.

No ledge exists at this point, and, instead of the firm stratum of gravel which we had hoped to find at the bottom of the excavation, a pocket of quicksand was found. It was by reason of the doubtful character of the earth that the writer adopted the form of structure herein shown.

The foundation was enclosed on all four sides with tongued sheet piling, as shown in Figs. 1 and 2. This was 6 ins. thick, about 16 ft. long, or driven as far as the steam pile-driver could force it, and the top

of this piling was held so it could not spread, and thus allow the sand within to escape. Resting on this sheet piling and locking over its top was a 3-ft. floor of old railroad rails and concrete, which in turn was stiffened by the stone piers above, acting as ribs over this floor, and, as a few other steel rails were inserted as ties in the masonry above the

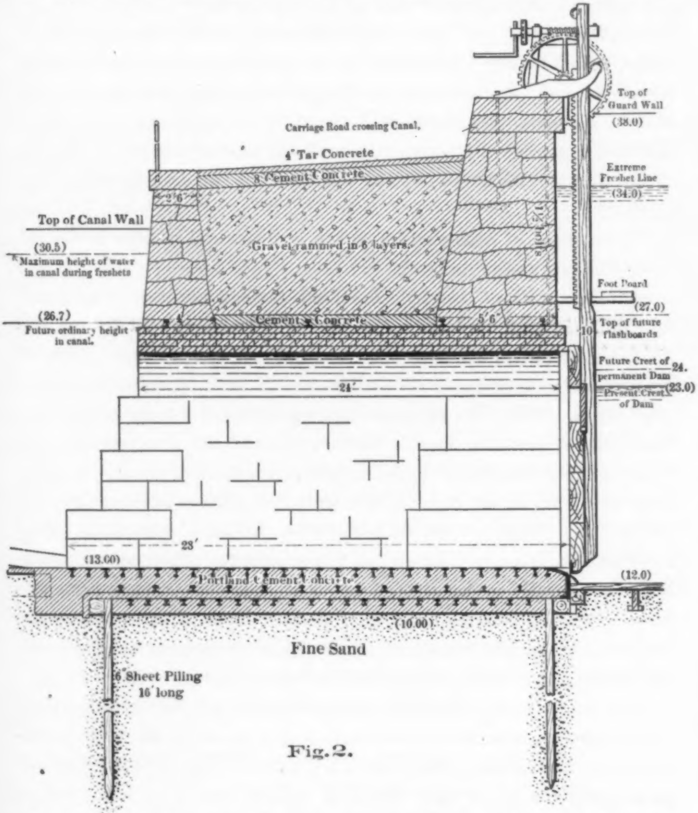


Fig. 2.

arches, the whole mass of masonry composing the sluices is expected to prove rigid as a monolith, and any settlement cracks are believed to be an impossibility.

Steel rails imbedded in concrete form a most excellent structural material for works of this class, and the writer has in several instances

felt very grateful to the engineers of Chicago for calling attention so conspicuously to its merits by their remarkable foundations.

Wing walls of cement masonry resting upon lines of sheet piling, carried down to a depth as great as could be conveniently driven, were, of course, extended out transversely to the axis of the canal for a considerable distance on either side of the sluiceway.

A plank apron, to resist scour, extends 25 ft. up stream from the gates, with 3-in. sheet piling driven 6 ft. deep across the channel at its up-stream end. A plank floor was designed to extend down stream from the concrete floor to a distance of 180 ft. from the gates. The writer did not supervise the construction of any part of the work, and has been told that this plank flooring was modified and decreased by those in charge.

The method of construction of the iron ties and girders within the concrete floor was as follows:

A 6-in. layer of English Portland cement concrete was spread and well rammed over the whole site of the sluices; on top of this a course of rails, placed on their natural bed, located 1 ft. apart on centers and extending the whole distance of 76 ft. transversely to the sluiceways. Then the spaces between were filled by a layer of concrete, and other courses of rails at right angles to the first were placed, running along under the edges of each pier. The outer ends of these rails were bent down, to hook over the guide timber of the piling and prevent any chance for the bulging out or spread of this piling; then another 6-in. layer of concrete, and then another course of rails running transversely to the sluices, but 2 ft. apart on centers; then two more layers of concrete, and then another layer of rails close to the top of the floor, laid bottom up, 12 ins. apart on centers, and flush with the flooring of concrete, which was rammed in between them.

The large proportion of iron which shows on the surface of this sluiceway floor, and the firmness with which the concrete locks in between the rails, gives a floor which it is believed will withstand almost indefinitely the scour from the swift current which at times rushes under the gates.

This mass of iron-ribbed concrete gave an excellent bearing and support for the narrow granite piers between the sluices, and the computed resultant pressure of a full pond with an empty canal, combined with the weight of masonry, falls within a base well suited to support

it, and which has furthermore a large margin, to take care of the incomputable stress from ice thrust.

The sluices are of rather coarsely dressed squared granite blocks laid in Portland cement.

Brick arches laid in Portland cement were used to cover the sluiceways, and imbedded in the masonry over these arches four more lines of old railroad iron were run, the outer ends of these being hooked down and adjacent ends strongly spliced, so that the whole masonry should be tenaciously bound together.

A comparatively small amount of cut granite was used, hardly any, except in the thin piers, the gate seats and the gearing bedstones, nearly everything being built from irregular waste stones, or "quarry grout," from the celebrated Concord granite quarries, which are about two miles distant.

It was intended, however, that much care should be taken in laying and bonding these irregular stones.

The sill on which the gates shut and the edge of which forms one blade of the powerful shears, intended not to be impeded by stray twigs of driftwood, is a massive girder-shaped iron casting, firmly imbedded in the concrete and extending under the piers. This is not machine-planed, but was cast with the foundryman's best effort to secure true form and smooth surface. The cast-iron pier faces, against which the iron face-plates of the main gates slide up and down, were also not planed, but the foundryman's skill was relied on to produce surfaces reasonably smooth and straight. At the top of the sluice the brick arch is faced by a cast-iron segment firmly imbedded in the masonry.

Money was scarce for the large enterprise in hand, and it was imperative that the design be such that the cost should be the absolute minimum consistent with safety and efficiency.

The actual cost for all of the excavation, sheet piling, old rails, concrete flooring, brick arching, cut granite, and, in short, for everything within the limits shown in Figs. 1 and 2, except the gates themselves and their hoisting rigging, and lying within the area, about 30 x 80 ft., occupied by the sluiceways proper, was about $15 000 00

The cast-iron pier faces for gates to slide against, sills and arch faces, including bolts, cost, for each gate erected in place, about..........................	$200 00
And are included in the total on page 283.	
Each gate with its trucks, paddle-gate, hoisting rigging, gearing, pillow, blocks and fittings, all erected and complete in place, cost, about........	700 00

All of which amounts, it may be remarked, were inside the preliminary estimates. The effort in the design was to reduce everything to the lowest terms consistent with prudence and safety, and to design the iron-work so that the pattern-maker and the moulder should do most of the manual labor upon it, leaving as little as possible for the machinist to do; and the cost, as stated above, may certainly be regarded as moderate for a work of this kind and size.

Each gate has a clear width of 10 ft., and a clear height of 12 ft., from sill to top of arch.

The feature of special interest and the sole *motif* for this paper is the method of reducing the sliding friction of the gate and bringing it within the strength of one man to lift or lower a gate quickly. We will, therefore, cull out from the dozen sheets of detail drawings enough to make the outlines of the apparatus clear, and to illustrate the method of its operation.

The writer had previously made an essay in reducing friction waste on a gate under a heavy load at the Nashua wasteway, described in a previous paper,* and for a time considered adopting there the wedge-track method here followed, but was drawn aside from doing so by certain considerations of no present importance.

The pressure which these gates at Sewall's Falls must always stand ready to carry and work under, with an empty canal and pond swollen by summer rains, amounts to about 40 tons against each gate. A prudent manager would not empty the canal during an extreme freshet, and, although the factor of safety would cover the stresses thus introduced, they may be dismissed from the present discussion.

Extended experiments by Hiram F. Mills, Civil Engineer (unpublished), have shown that values given by eminent authorities for the friction of cast iron on cast iron without any oil for a lubricant, and

* Trans. Am. Soc. Mechanical Engineers, Montreal Meeting, 1894.

repeatedly quoted in the various books of engineering data, are too low, the iron then under experiment probably not having been absolutely free from oil, and considering that we here, from motives of cost, relied, not upon machine-planed surfaces, but upon excellent foundry work, with good plumbago foundry facing to make the contacting surfaces of our slide plates smooth, it is probable that the frictional resistance to moving the gate, together with three tons for the weight of the submerged gate and its attachments, might, with an empty canal, be as high as twenty tons.

It should be possible to shut any canal head-gate with a high river on the up-stream side and an empty canal below, for if a washout or serious break occurs, how else is the flood to be quickly controlled?

Here, we may be pardoned for remarking that the existing head-gates with gearing and mountings sufficiently powerful to fill this requirement are very rare.

We made this gate go easy by carrying it upon the four cast-iron trucks as shown in Fig. 1. These trucks are 18 ins. diameter, and run on a cast-iron track formed in the same piece with the pier-face.

During the entire time when the gate is raised from its sill, the pressure is carried upon these trucks. When the gate is entirely closed, these trucks are thrown out of commission by a sloping depression of about half an inch in the tracks upon which they run, which allows the gate to drop back into close contact with the faces of the pier and sill. These wedge-like inclines in the track are shown at w and x (Fig. 1).

Suppose the gate now to be closed, and that we start to hoist it. At first it will start with no less of an upward pull than the ordinary gate, but by the time it has (by one minute's use of a long lever, inserted in the notched wheel M, Fig. 5, instead of the hand crank) been laboriously raised from 1 to 2 or 3 ins., the wedge-shaped incline on the rail under each of the four trucks will have brought these trucks to a firm bearing on their rails, and the face of the gate will be lifted up stream ¼ in. or ½ in. away from sliding contact, and sliding friction will have disappeared, and thenceforth the progress of the gate will be rapid and easy.

Should one fear that drift matter may lodge between the gate and the pier, and, by holding the gate off its seat, cause leakage when it is

shut, we may remind him of the powerful grinding action of the iron plates when moved under this great pressure upon the pieces of foreign matter, which would quickly reduce them to powder.

These wedge-shaped depressions of the truck rail are $\frac{4}{10}$-in. for the upper pair (at *w*, Fig. 1), and $\frac{5}{10}$-in. for the lower pair of trucks (at *x*, Fig. 1), these being so proportioned in order that no tendency to move the gate-start up stream or down stream would exist at the point where it engages with the pinion.

In other words, the gate-start swings or pivots about the pinion, when the trucks mount the wedge-shaped lifts upon their tracks.

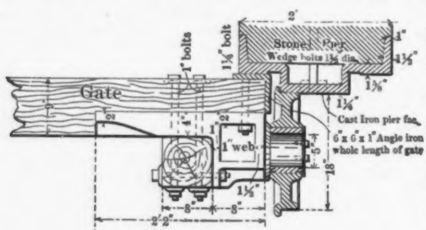
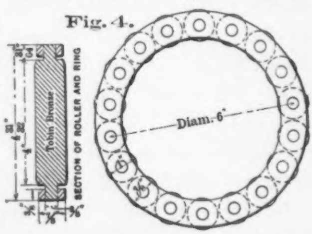


Fig. 3.

These trucks may have to carry each a 10-ton load, and their axles, to withstand so great a cross strain, must be massive and strongly secured to the framework of the gate. These axles and connections are shown in outline in Fig. 3. It will be observed that the gate-starts, so-called, or the massive handles by which the gate is pulled up or pushed down, are so connected to these axles as to distribute the stress, and that the angle iron edge of the gate gives it further stiffness.

Fig. 4.



The 5-in. axle in an 18-in. wheel would, without special appliances, in turn have a good deal of friction, being under water where oiling is impossible, and where fine sand in the water sometimes abounds. We, therefore, introduced between the axle and the truck, the roller bearing shown in Figs. 3 and 4.

These rollers are of Tobin bronze rod, 1 in. diameter, turned down to a $\frac{3}{8}$-in. neck at each end, to form a journal, which rests in a corresponding hole in a brass ring or collar, thus holding the several rollers in proper relation to each other. Their working face is 4 ins. long, and there are 18 in a nest.

Passing now to the top of the structure, the gearing is shown in

outline in Fig. 2. The worms are shown on a larger scale in Figs. 5 and 6.

Each worm was cut from a solid shaft of forged machinery steel 4 ins. in diameter. Since the worm tooth engages with a cast-iron tooth of the worm wheel, and since the short time of working will produce slight wear on the worm, we favored the weaker metal by making the wrought steel worm tooth one-eighth thinner than the cast-iron gear tooth, desiring to add all that was possible to the strength of the cast tooth, because worm wheels on gate hoists are so often found with broken teeth.

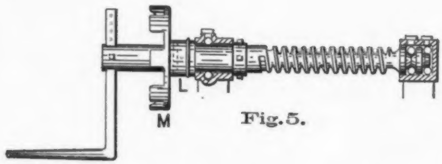


Fig. 5.

It will be noticed that the worms have a much smaller diameter than the time-honored custom of old millwrights would call for; and by so simple an expedient as reducing this diameter, bringing the friction nearer the center and getting a greater leverage on this friction with the crank, we save about half the friction found in the majority of worm-gear gate hoists.

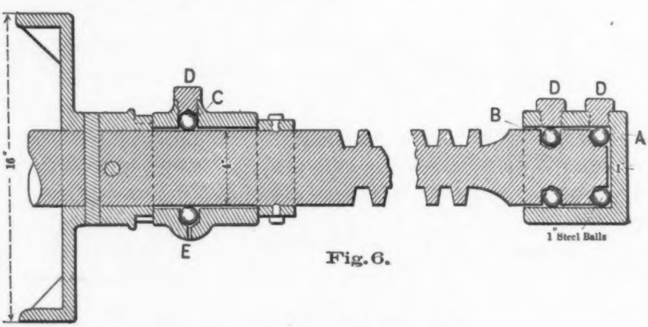


Fig. 6.

This worm has a diameter at pitch circle of gear of only twice its pitch (or more exactly, diameter of bottom of thread is $2\frac{1}{16}$ ins.; diameter at top of thread, 4 ins.; diameter at pitch line of wheel, $3\frac{1}{8}$ ins.; pitch, $1\frac{1}{2}$ ins., and angle of advance, nearly 15°), but it works

in an entirely satisfactory manner, notwithstanding its sharp angle of advance. The only trouble thus far found is that the worm wheel arms are not so stiff as they ought to be in a direction parallel to the axis. Therefore, when the worm is not properly lubricated, or when working under a heavy load, the rim of the worm wheel springs off to one side a little, under the drag of the worm, and then jumps back.

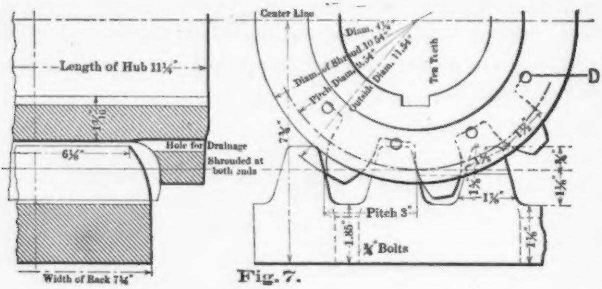


Fig. 7.

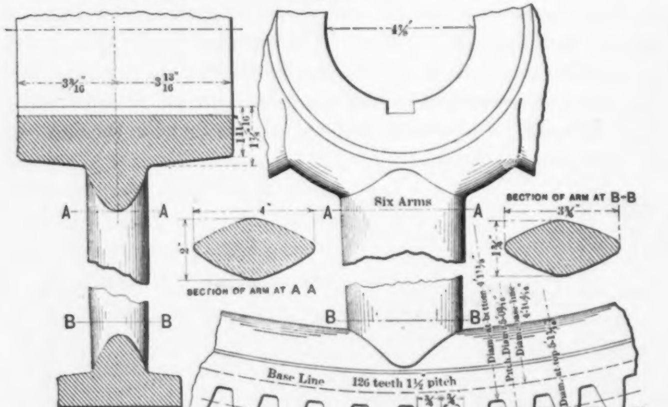


Fig. 8.

In another design the spokes of the worm wheel ought to have stiff ribs added to the rather thin section shown in Fig. 8, to prevent sidewise distortion.

To retain sufficient torsional strength with this small diameter, we had to call the worm screw thread itself into play in adding to the torsional strength of the shaft, and therefore at the crank end of the worm the screw thread starts in from the 4-in. cylinder without any notch, as shown in Figs. 5 and 6, instead of beginning the cut from a notch, cut straight around, as is more common machine-shop practice.

As a further means of relieving friction, and as a respectful recognition of the fad for ball-bearings which is abroad in the land, we worked in quite a number, as shown in Fig. 5, in small scale, or more in detail in Fig. 6, and we firmly believe them to be a very valuable acquisition for this kind of work.

The balls A take all the thrust, and B and C the side load. They are inserted by unscrewing, for the moment, the plugs D, which were, of course, in place when the cylinder and the ball channels were bored out. The hole E affords drainage. It will be noted that the boring work is of a simple kind, cheaply done.

The balls themselves are inexpensive (these balls, 1 in. in diameter, cost about $2 50 per dozen). As will be seen from the drawing these bearings may stand out in the weather and run small chance of damage from rust or ice; moreover, they start easily in a sudden emergency, although oiling has been long neglected, and what is noteworthy, considering their many advantages, these ball-bearings cost only a very little more for shop work than the simplest ordinary form of babbitted journal.

Trouble from lack of long-wearing qualities does not arise in this class of mechanism; it moves so slowly and so comparatively seldom, or is in motion for such short periods.

Broken pinions and rack-teeth are so common on heavy gate hoists that we may be pardoned for presenting such simple machine details as shown in Figs. 7 and 8, which exhibit the forms and dimensions of those designed for this work.

This involute rack-tooth, with its great thickness at the root and the pinion shrouded at both ends, gives a strength ample for any computed stress due to the use of the gate, and a fair margin for such abuse as is sure to come once in a while when things are ice-bound, or a piece of water-logged driftwood gets under the gate. As has been said, $P +$ "Pat." must then be the value for use in our formula for stress instead of P alone, as given by the books.

Fig. 7 thus registers the writer's estimate or guess of the fair allowance to be made for "Pat."

The holes *D* are to allow water from rain or melting snow to escape, and were put in to escape a difficulty in which the writer once got caught in an earlier design where ice was formed in the pocket between double-shrouded pinion teeth, preventing working the gate until it was chipped out.

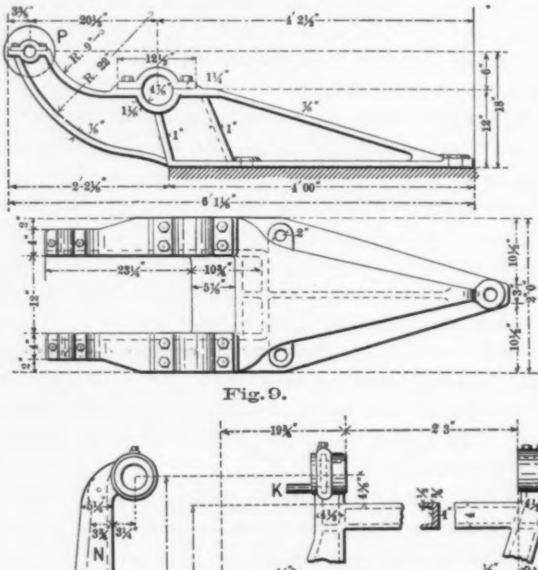


Fig. 9.

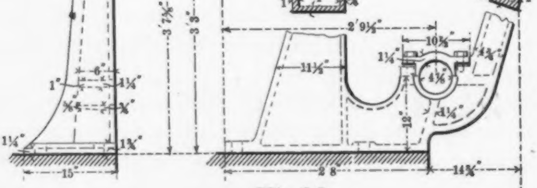


Fig. 10.

Figs. 8, 9 and 10 are other details, and illustrate the precautions taken to hold all bearings rigidly in line. The writer has ventured to trace Figs. 9 and 10 from a later design for another site, in which

certain slight changes were made in the patterns to improve the appearance and stiffness of the castings.

The front tie-bolts, 1½ ins. in diameter and 12 ft. long, holding down the pillow block of Fig. 9, were designed to be carried down to the great depth shown in Fig. 2, in order to suspend a sufficient weight of masonry therefrom, to hold the pillow block from lifting when the whole power of the gearing should be exercised in forcing down the gate against some possible obstruction.

The corner of the rack-tooth was rounded off, as shown in the longitudinal section of Fig. 7, in order to strengthen it by bringing any uneven strain back further from the end of the tooth.

It may be remarked, furthermore, that the power and strength of all the gearing was computed on the basis of forcing down a wide, open gate against a pond swollen by a summer flood, with an empty canal (due to some unexpected breach of the canal embankments), and that the necessary strength was computed on the hypothesis that the friction trucks on the main gates were not working, and that the roller bearings were inoperative, and the paddle-gates not opened.

In other words, the safety of the works was assured without these friction-saving devices, which were regarded as auxiliaries for saving labor.

The "paddle-gates" (Fig. 1, *A* and *B*) are for use when filling the canal after it has been emptied, and through them water is let in to balance the pressure on the main gates, much as a by-pass is used on very large water-pipe gates. They were placed at the top of the sluiceway under light head, that they might be easy to open and shut, and with the further object of avoiding the tendency to scour upon the canal bottom just below the sluices if filling the empty canal with water admitted under high velocity through a smaller opening near the bottom. With these paddle-gates, the first water falls vertically on the masonry sluice floor, and quickly has the energy for scouring taken out of it, before it reaches the wooden apron or sandy bottom a short distance down stream from the sluices.

Only three of the main gates are yet provided with these paddle-gates. These are each 5 x 2 ft. in the clear, and are of a total capacity sufficient to fill the empty canal in from about half an hour to an hour, according to level of water, and according to the leakage at the turbine gates. Of course the filling can be accomplished as rapidly as desired

by lifting one of the main gates, after the bottom of the canal has, by means of the paddle-gates, been covered 3 or 4 ft. deep to lessen the scour.

The hoisting rigging for the paddle-gates *C* (Fig. 1) is somewhat novel, and is carried upon the main gate-starts as shown in outline in Fig. 1. It is very simple, consisting of the notched wheel *E*, which is turned by a hand-spike or wooden lever 4 or 5 ft. long; on each end of the same shaft a 12-tooth pinion is carried which engages with a cast-iron rack of 1½-in. pitch and 3-in. face.

The starts *D* for these paddle-gates are of 2½ x 2½ x ¼-in. angle iron, and the angle of each lies compactly against the corner of the main timber gate-starts of the main gate, in order to lessen the possibility of its being knocked out of shape by an ice jam or by floating drift-wood. It will be noted that the whole of the main gate, including the trucks, can be run up easily for repairs above ordinary level of water in the pond.

These main gates are of Georgia pine 9 ins. thick throughout the lower half, and 7 ins. over the upper half.

The several planks are tied together by two large and tightly set bolts (*FF*, Fig. 1), 1½ ins. diameter, extending from top to bottom; also, by the two 8 x 10 gate-starts of Georgia pine, which furthermore serve as girders for stiffening the gate against the pressure of the trucks. The 6 x 6 x 1-in. angle irons at each end which serve as slide plates also stiffen and strengthen the gate.

A facing of 1-in. iron plates, 6 ins. wide, goes all around the edge of the wooden gate, to form a good wearing surface, and to make a securely tight joint with the iron plates facing the sluiceway masonry. For a head-gate joint, wood on iron, or wood on stone, might be the dictum of the old millwrights, but the writer believes that for durable working faces in sliding contact, iron on iron is far the best. Furthermore, it is the writer's belief that a water-tight gate can be secured more certainly where the lower edge shears past the edge of the sill than when it merely presses down upon the top of the sill.

The shearing force available from the weight of the gate and the power of the rigging is easily 25 tons.

In order that the middle portion of the edge of the gate might not lock in and become blocked against the corner of the sill, from being sprung inward a little by the pressure of the water, we made the top

corner of the massive cast-iron sill, already described, not horizontal, but slightly V-shaped, as shown in Fig. 1.

The computed weight of one gate, with its attachments, is about 5 tons in air, and, when near the bottom, the buoyancy of the water upon it will be about 2½ tons. The providing of two 2½-ton counterpoises for each gate was considered, and the works so designed that they can be added if they should ever be deemed advisable. The eye-nut *G* on the top of each main 1½-in. dowell bolt which extends from bottom to top of the gate furnishes a convenient means of lifting the gate by chain-falls in case of any accident or during repairs, and was also designed as a place to attach a counterpoise chain, which should run over elevated pulleys and lead to a counterpoise suspended above ordinary high water.

The writer considered, however, that a counterpoise was not worth its extra cost, and that the loss in lifting would be offset by the ability to let the gates run down rapidly under their own weight controlled by the simple friction brake, composed of an elastic oak lever about 1 in. thick by 3 ins. wide by 4 ft. long, resting on the fulcrum *K* (Fig. 10), and pressing against the worm shaft at *L* (Fig. 5), the outer end being held in the hand. From examining the finished structure (for, as already intimated, the writer did not personally supervise the construction, having undertaken the design with that understanding by reason of other engagements) this little detail of providing for a friction brake happened to get overlooked by the millwright.

This simple kind of a friction brake has been in satisfactory use at the north canal head-gates, at Lawrence, Mass., for many years, and it is found a great convenience to be able to let the gates run down much of the distance under their own weight; so much so that a counterpoise would appear objectionable rather than advantageous.

The worm-shaft stands were so designed that, should it ever be desirable to work these gates (or others with gearing cast from the same patterns) by electric power, for regulating the height within the canal, this could be simply arranged by bolting a worm-gear rim to the rim of the notched wheel *M* (Fig. 5) and driving the worm which engaged with it by a small electric motor which would be bolted to the face of the stand, shown in Fig. 10, at *N*, and run at high speed under control from a switchboard in the electric power station at the foot of the canal.

In this particular case the contingency of ever wanting any electric power attachment is very remote, since the extra height of the canal banks avoids the necessity for such close regulation as at most other powers.

The reason for placing the roller *P* (Fig. 9) at a higher level than the pinion is to facilitate the slight swinging off of the main gate as its trucks mount the wedges of their track.

The ratios of the train of gearing are such that one revolution of the pinion raises the gate 30 ins. and this calls for 126 revolutions of the crank, or one revolution of the crank lifts the gate very nearly one-fourth of an inch.

From 30 to 50 revolutions per minute can readily be made on a crank of this sort, and at 40 revolutions per minute the gate would be raised 10 ins. per minute.

DISCUSSION.

CLEMENS HERSCHEL, M. Am. Soc. C. E.—The hoisting apparatus described by Mr. Freeman shows many refinements of construction put upon their good behavior in practice for the first time. Some of them are, in my opinion, unquestionably good construction, and will probably wear well; others I do not think are sound and good. It is pleasanter to refer only to the first class; but the latter kind should be referred to also. I place among the former the adoption of the Chicago foundation construction for the foundation and covering of the gateway arches; the details of the worm and pinion mechanism, including ball-bearings, steel endless screws, and general disposition of this apparatus, though I doubt, at the same time, the superiority of it over a screw and nut mechanism for lowering and hoisting.

Having a mechanism that admits of the possibility of letting the gate run down free, or only controlled by a hand lever, I also have my doubts about. I prefer a mechanism that holds the gate in place wherever it may happen to stand. The V-shaped sill is a neat device for preventing the catching of the middle of the gate on top of the middle of the sill, but my preference would be to have no rebate sill, and, instead, to have the gate shut down flat on top of a prepared flat sill. The object of the rebate is stated to have been the readier attainment of a water-tight joint; but I think this construction will tend to make difficult a shutting down of the gate water-tight, or anywhere near water-tight, by the lodgment in the rebate angle of sand, stones,

and similar trash, which the increased current when closing the gate will not dislodge; while a flat sill and gate can always be made water-tight by throwing a half dozen shovelfuls of ashes, or shavings mixed with gravel, on the up-stream side of the gate, after it is shut down as low as it will go, or nearly water-tight.

The fastening of these head-gates to roller wheels is a radical departure from accepted mill practice, which does not promise well in my opinion. Several constructors have tried to reduce the great sliding friction of head-gates by devices of this sort hitherto, on works already in operation. Mr. C. J. Cheney, of Lowell, some 40 or 50 years the millwright employed under our Past President, Mr. James B. Francis, designed and patented an arrangement of this sort about 10 years ago. He loaded the gate and the pressure upon it on a four-wheeled truck, as it were, by lifting the gate from its contact with its bearings when shut, and so as to bear on the axles of the truck, by means of four eccentrics on these axles, operated by an endless screw from above water; and in closing the gate he reversed the process, and unloaded the gate from the truck, back on to its gate-bearings.

Mr. Stoney, of England, has designed several sluice-gates on navigation works, among others several on the Manchester Ship Canal, some of them of very large dimensions, in which the gates have constantly between them and their gate-bearings roller-carriages like those under the movable ends of bridges. Of course such an arrangement cannot make a water-tight gate, which does not matter on navigation canals and at the points of water waste where these gates have been used.

Still more important is the consideration that such gates are not used in the winter, when navigation is suspended, while the winter time is the period of the most difficult service in the case ef water-power plants. In England the winters are also excessively mild, judged by our standards, so that many a contrivance used on hydraulic works in England becomes worthless when transplanted to the New England or other northern States of the Union. Many a New England engineer has probably looked with envy on the kind of grass-hopper structures that do service all the year around in Southern California, Arizona, New Mexico, not to mention Southern Italy and Andalusia, and has wished that he too might for once be rid of being obliged to design and build what seems foolish in the summer, when the work was going on, and the uses of which would only appear in the succeeding winter when he very likely would be alone in the struggle with the operation of the works.

But winter must be reckoned with, in our climate, and among its products is ice—ice in cakes, ice as slush, ice as anchor ice, ice on the surface of the water, ice in the water, and hard ice forming on iron and stone surfaces below water. The condition that our hydraulic

structures must all fulfill is that they must be operative, without breakage, in the coldest weather and under all conditions of the formation of ice; and I think it will be found that only exceedingly simple, or crude and clumsy structures, if anyone choses to call them so, will live and survive under those conditions.

I am aware, for example, that various contrivances have been gotten up to make a Stoney gate water-tight, in spite of its being mounted on roller-carriages, by Stoney himself and by others, so that they could be used on works other than navigation sluices, for which they were originally designed. These devices take the form of corner-beads and the like, say, like the weather-strips against windows, that keep out the winter's blast. But I have not heard that they have done their office during a winter in a climate like that of our northern States. In my opinion they should be expected to fail in operation during the winter.

Mr. Freeman, you will observe, gets over the difficulty of such leakage by letting the gate when shut run off its roller wheels, and restoring to it its bearing on the gate-bearings. This makes it water-tight when shut, or nearly so. And Mr. Cheney did the same thing in another way.

Now the only justification for doing anything of the sort, for taking any thought of the amount of resistance caused by the sliding friction of the gate on its bearings, for running any risk in the operation of the gate while trying to lessen this resistance, as well as for spending money in such a cause, would be a lack of power to overcome this resistance, as is found when men have to be sent to shut down, or to open up, the gates; but as is not the case, whenever water or other power is on hand or can be transmitted to the gate-house.

Let me give a homely illustration. A common drag for stone, or a stone-boat, as it is often called, with a yoke of oxen hitched to it, is a common sight on public works or on the farm. A boulder is rolled upon it, and the yoke of cattle plod along. They strain at the yoke, step by step, with their heads held low, the foam running out of their mouths in long strings, until one would think they are going to choke or until they actually do get stalled. Now suppose another yoke of cattle to be hitched on. These four will walk right along with the same boulder, step by step, in an easy swinging walk; and if two more yoke of oxen were hitched on, the show would consist of a pebble on a chip on the end of an ox chain, with hardly enough weight on the stone-drag to keep the chain taut, and the drag from swinging sideways back and forth. It is the same with a head-gate sliding on its gate-bearings; if only enough power be applied, its motion, whether of iron sliding on iron, or iron sliding on stone, or iron wheels rolling on iron, will not give a moment's care or even thought to the parties in interest, and will not authorize or justify the expenditure of a dollar

or the running of the slightest risk in the operation of the gate for diminishing friction.

Such abundance of power is generally at hand in the case of water-power plants. It is not at hand, as a rule, in the case of navigation works, which explains why these gates, running on roller-carriages, had their origin as navigation sluices. This sort of works is, as a rule, placed at a distance from towns or villages, at some lonely spot, with only a gate-keeper, his wife and a family of small children to attend to them. And a gate that he alone, or his wife in case of necessity, can operate, becomes a much better appliance than one that requires 20 or 30 or 40 H. P. on the shaft that moves the gate-rods.

In the head-gate houses of most water-power plants, there is usually placed a turbine water-wheel to operate the gates. This must be very carefully set so as to be operative under all conditions of ice, which can generally be done with steam piping, to melt out the anchor ice as a last resort. I have known a steam engine to be kept in reserve, with clutch couplings to make the changes. Also a steam engine to be used alone, or in conjunction with a reserve steam engine. One of the sets of head-gates at Lowell uses hydraulic pressure from the fire protection reservoir. And in these days of electric long-distance power transmission, to locate 20 or 30 or 40 H. P. at any head-gate ought not to be a difficult matter.

Such power is used, as a rule, but seldom ; only at long intervals in the generality of cases. But care must be taken, by duplication of machinery, or otherwise, that the power does not fail when wanted.

I think, therefore, that a better form of head-gate machinery would have resulted from supplying more power at the head-gates in question and having the gates plain sliding gates. And I doubt the working of the appliances used to diminish sliding friction when the gate, guides and wheels, and all the under water iron-work, will be encased in the hard ice that forms on such surfaces in very cold weather. In that event, one of two things is liable to happen, in my opinion. The gate will get jammed, so that it cannot be moved, or else something will break, neither of which eventualities is to be desired.

Finally, if mechanical power could not, under the appropriation for the gate-house, be applied, the gate could always have been made to move so slowly that one man could easily operate it. I built a gate of that sort at Holyoke, 14 ft. wide in the clear and 6 or 7 ft. high, a very large gate. One man opened and shut it, with no back pressure on the down-stream side to aid him, with one hand, but he had plenty of time to reflect on the mutability of human events, or on any other philosophical subject while he lowered or raised it 1 ft. But it did its work. In the winter time, when it jammed in the guides, a liberal application of kerosene oil would sometimes have to be used to rot out the attached ice. Moving the gate up and down could be resorted to,

to keep it in operation in excessively cold weather. It was nothing but a platform of 12 x 14 ins. hard pine, doweled, through-bolted, iron-bound, and having a slow but powerful gearing attached, to keep it going. I think it was a better gate for the place than would have been one modeled after the pattern of those here discussed. I think it was a better kind of gate machinery and running gear to have set up at the Sewall's Falls head-gate house, though better than either would have been some sort of mechanical power, plenty of it, according to the speed at which it was desired to move the gates and simple sliding gates. I say better, because I think such an apparatus would have been less likely to refuse to do its duty, at the time of some extraordinary occasion, which may be expected to occur upon any day during the severely cold weather that tries our work as the winters come around.

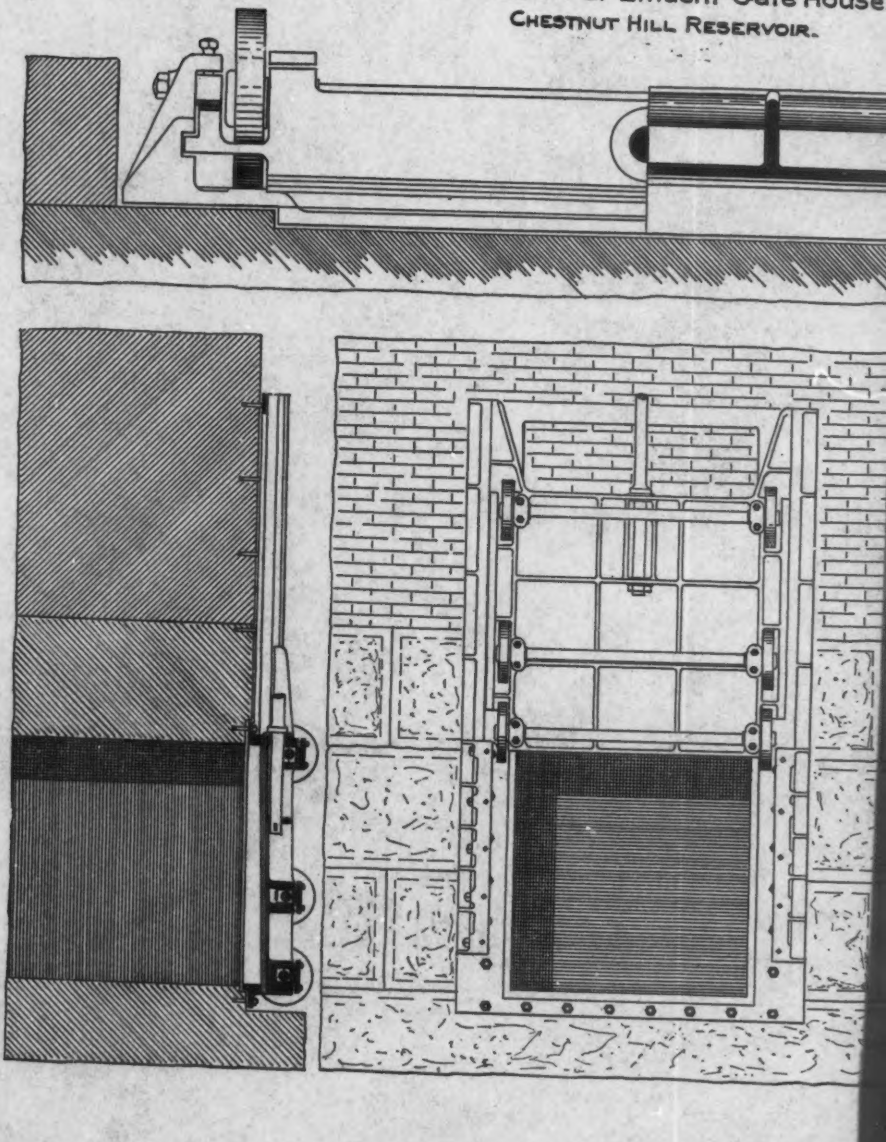
DESMOND FITZGERALD, M. Am. Soc. C. E.—This paper of Mr. Freeman's has interested me very much, principally because nearly 20 years ago I had occasion to design two gates for the city of Boston running on wheels. They are now in use at Chestnut Hill Reservoir. Although much smaller than those under discussion, being only 4 x 4 ft. opening, yet it is interesting to see how a problem is worked out on similar lines, but entirely independently, by two different people. Plate XLIII shows the Boston gates to which I have alluded. The stems are connected at the level of the floor of the gate-house with pistons in cylinders, which are filled with a mixture of glycerine and water. The gates run down of their own weight by turning a couple of valves, thus allowing the fluid to run from the under side of the piston to the upper side. The gates are raised by a hydraulic press, worked by man-power, and the gates can be raised quite easily with one hand.

It will be noticed in the design that the wheels are mounted on axles which run entirely across the face of the valve. The frames and valves are of cast iron, the latter heavily ribbed, to sustain the pressure of 25 ft. of water, under which they work. The sides are wedging in form, and just as the gate rolls to the bottom and is on the point of closing, the wedging action comes into play, the wheels rise from their track, and the brasses upon the valve and the valve seat come into contact. The gates are almost perfectly tight, and they have been working without any repair whatever for 20 years, and I have no doubt they are good for a much longer time than that in the future.

It seems difficult to understand how any one could secure a patent on a gate running on wheels when they have been made for so many years. I never saw one before mine were built, but the idea was given me by Mr. J. P. Davis, and I dare say that he received the idea from others. In this design Mr. Freeman has provided a very cheap construction combined with strength, particularly as far as the masonry is

48 inch Gate at Effluent Gate House

CHESTNUT HILL RESERVOIR.



48 inch Gate at Effluent Gate House
CHESTNUT HILL RESERVOIR.

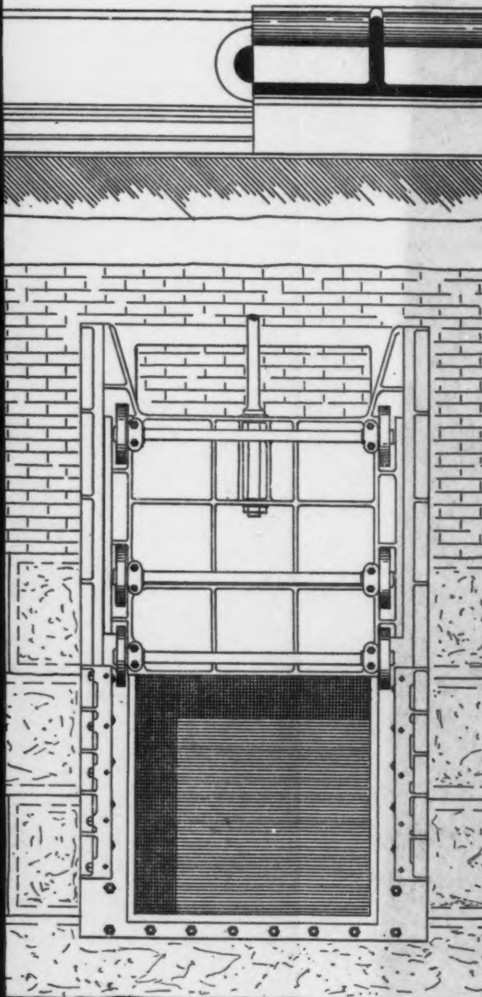
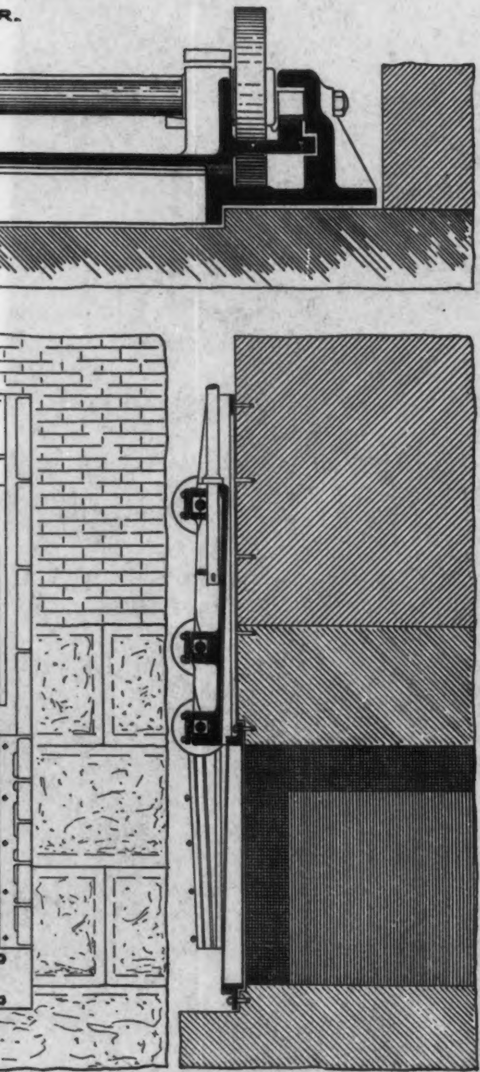


PLATE XLIII.
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXXII, No. 733.
FITZGERALD ON HEAD GATES.

House.

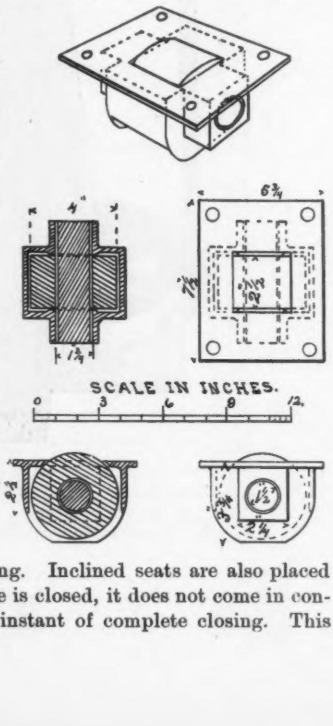
R.

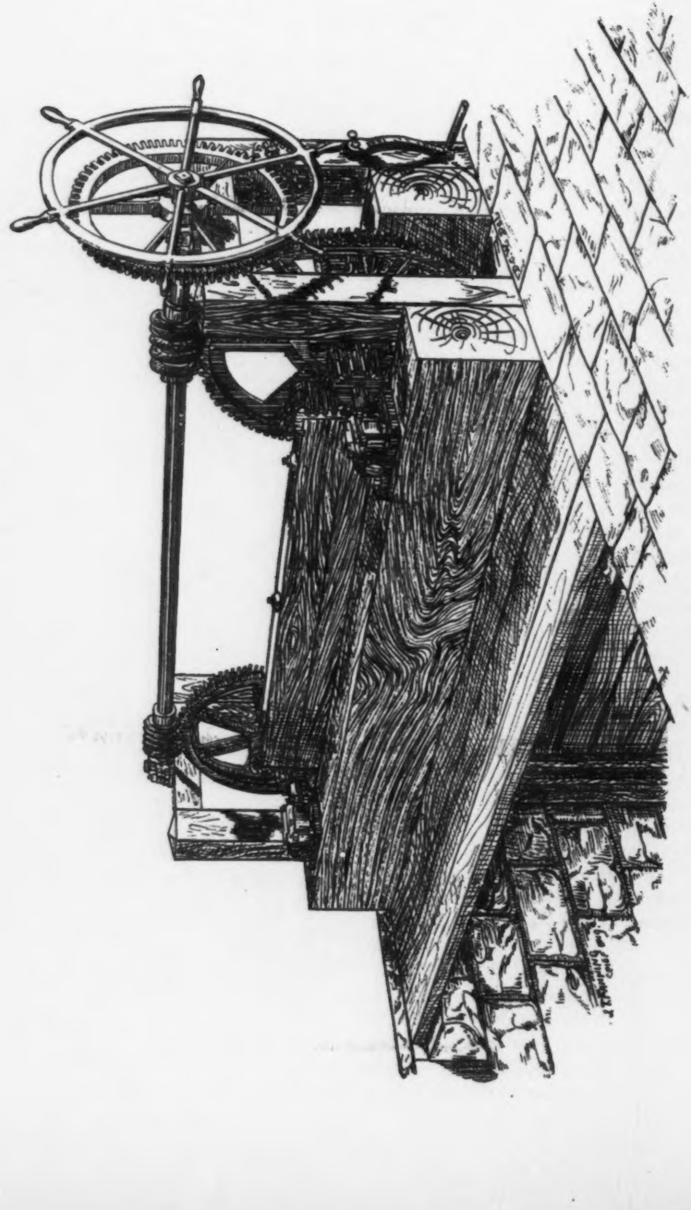


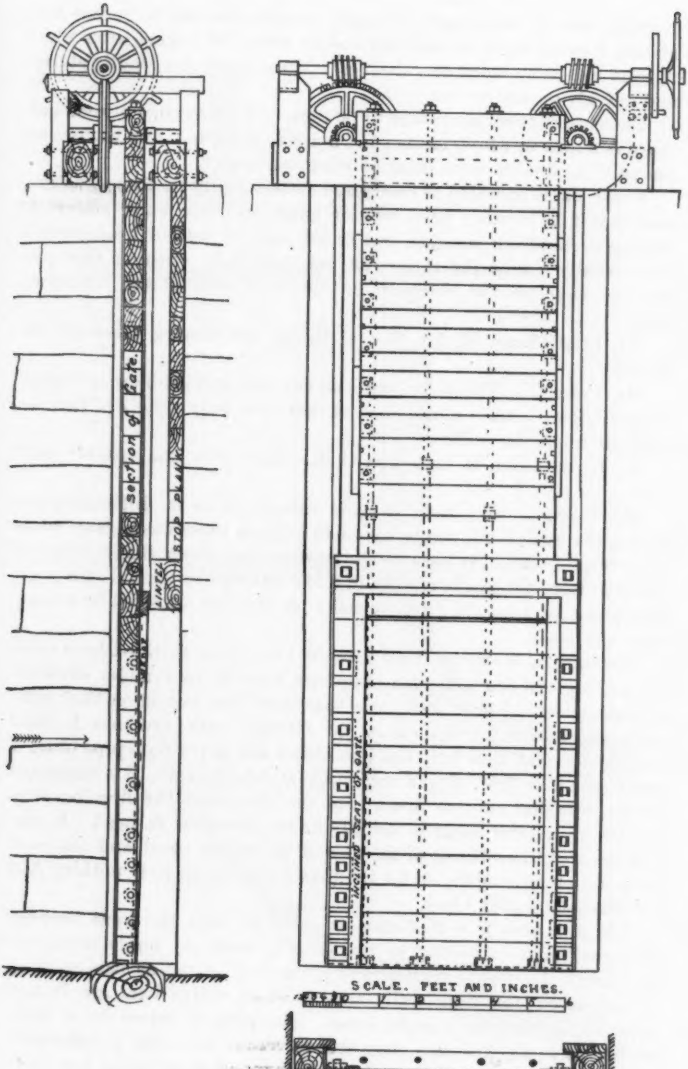
concerned, avoiding expensive cut stone. I am not quite sure in regard to the provision of power for working the gate. We have always found the friction of gearing very much larger than any calculations would give, but the practical working of the gate ought to show any weakness in this direction. Plate XLIII shows the details of the Boston gates so completely that I will not enter into a further description of them.

J. T. FANNING, M. Am. Soc. C. E.—When rebuilding a part of the dam at St. Anthony Falls, in Minneapolis, eight years ago, we found it advisable to put in waste gates in two locations. At one point there was a pair of gates; at the other, a single gate. Each of these gates is 6 ft. in width and 10 ft. in height, and the gate-sills are 16 ft. below the crest of the dam. Each of these gates has upwards of 20 tons water pressure upon the up-stream side, and no counterbalancing water pressure on the down-stream side. The sliding friction of these gates would be very great without rollers, and friction rollers were consequently planned for them.

On comparing Mr. Freeman's friction rollers with these, I observe that he used four rollers to each gate, one on each angle, and that his rollers are about 18 ins. in diameter and have axles 5 ins. in diameter. A larger number of friction rollers of smaller diameter were placed in the Minneapolis gates, nine rollers on each side, and so spaced as to distribute the pressure equally upon the sheaves. These sheave rollers are 4 ins. in diameter, and their axles, bushed with bronze, are 1¾ ins. in diameter. The form of each sheave is much like that of the window weight pulleys in a window jamb. They are placed on the bearing face of the gate. A plate of iron is fastened on the face of each gate jamb, for these friction rollers to move upon. These bearing plates are vertical and the rollers project ½ in. over the face of the gate. On the bearing face of the gate there is an inclined seat, tapering 1¼ ins. in 10 ft. height of the gate-opening. Inclined seats are also placed within the gate jambs. As the gate is closed, it does not come in contact with its seat until just at the instant of complete closing. This






Section of Gate.
STOP PLANK
LINTEL
INCLINED SEAT OF GATE.
SCALE. FEET AND INCHES.
INCLINED SEAT.

inclined seat gives a tight gate and permits the gate to be free from sliding friction upon its seat the instant after lift begins, after which it rolls up easily upon the sheaves. These gates are readily started and quickly opened by one man.

The cost of these sheaves is $1 50 each. On each edge of each gate there is a rack in which meshes the hoisting pinions. The pinions are operated by double worm-gear hoisting machinery.

These gates operated so easily and satisfactorily on first construction that I have since used similar gates on other water-powers in various parts of the country, but in the case of sets of head-gates, I have usually placed the rollers on one gate only, opening this gate first, to equalize the pressure on the two sides of the remaining gates.

Mr. FitzGerald, M. Am. Soc. C. E.—Is the bearing taken off the wheels ?

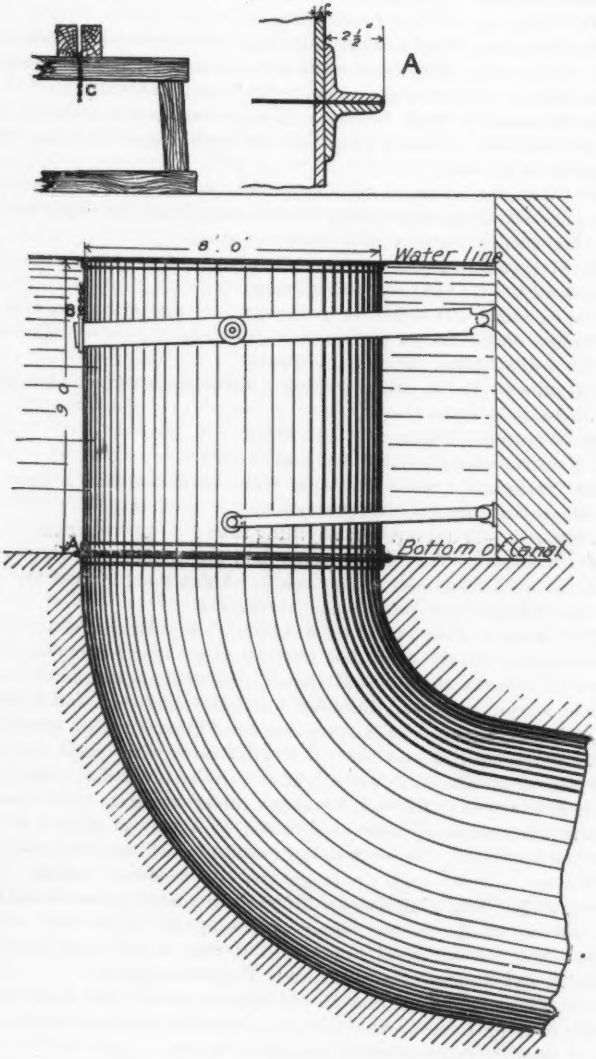
Mr. Fanning.—The wedge seats on the gate and jamb are in contact while the gate is fully closed, and at that time only relieve in part the pressure upon the rollers.

K. E. Hilgard, M. Am. Soc. C. E.—Have you had trouble with ice ?

Mr. Fanning.—We have to open various gates at different times during the winter, but rarely have had to open these waste gates while the river is frozen. In case we did have to open them, the ice could be removed from the jambs readily in a few minutes by the aid of a portable boiler kept for the purpose and a jet of steam, directed by a hose, upon the jamb.

William E. Worthen, Past President Am. Soc. C. E.—I have tried the principle of the bath pipe plug from time to time under different circumstances. I have seen gates that have been put in on that principle, and I have found them in the German works, so that I claim nothing for that idea, only the idea that I saw in the bath pipe plug. I put a gate in in 1888. It is a large bath, standard waste. It is connected with a pipe that extends down into the river, and the flow between the river and the canal is usually under about 35 ft. head. It discharges about two-thirds of the flow of the whole canal and has been in operation since 1888; so far the plan for hoisting it is nothing but a differential pulley block.

The pipe plug is 8 ft. diameter and 9 ft. high, the joint between plug and pipe extending to the river is made by angle irons, the movement of the plug vertically is controlled by radius bars working in centers on the back wall; each level of radius bars are framed strongly together with angle irons. The plug is raised by a chain hitched at *B* (see drawing, page 303) in connection with a differential pulley block suspended from *C*, and is dropped in the same way on to its horizontal seat.


C
$2\frac{1}{2}''$
A
8' 0"
Water line
B
9' 0"
Bottom of Canal

Mr. HERSCHEL.—Is that used in winter?

Mr. WORTHEN.—All the time.

Mr. HERSCHEL.—Suppose the bottom gets covered with anchor ice?

Mr. WORTHEN.—They have made a house over it, and are using it all the time for the last eight years, but not daily, I think.

Mr. HERSCHEL.—There is such a thing as anchor ice, and it would adhere to the seat of such a gate; how can such a gate be used when anchor ice is all over it?

Mr. WORTHEN.—A steam pipe would take care of that easy enough. I have never had any information from it except that the thing works; and when a thing works, that is what you want.

Mr. HERSCHEL.—I question whether it has been working in the winter with anchor ice running in the stream.

Mr. WORTHEN.—It will work in the summer time, of course, and we could clean off the anchor ice if it is there. A steam pipe will blow the ice off. I don't think there is any trouble.

A MEMBER.—Mr. Worthen, I think that bath plug of yours has been patented a good many times.

Mr. WORTHEN.—I have no doubt of it.

A MEMBER.—How much did it cost?

Mr. WORTHEN.—I made it of that size because we had a piece of pipe about that diameter and pretty long.

A MEMBER.—It did not cost anything then?

Mr. WORTHEN.—They did not complain about the cost; they used up all the old iron as well as they could. I don't know what the old pipe was intended for, but it came in very well.

T. C. KEEFER, Past President Am. Soc. C. E.—In connection with this subject I should state that over 50 years ago, when they first proposed to make the Welland Canal locks of stone instead of wood, Colonel Macaulay, Royal Engineer, was Chief Engineer, and I was a young man under him. His plan, instead of filling through the gates, as the old canal did, was the old English lock, with breast wall and land tunnels in the side walls, which were square holes, both horizontal and vertical in the wall, forming a culvert along the whole length on each side, and these holes were closed by a bath plug such as Mr. Worthen speaks of. It was a square, solid plug of oak, that could be carried up in layers until the height gave the necessary weight. The bottom of the bath plug was an inverted truncated pyramid, and the seat corresponded. It was square and sloping. This was not an original plan of the Royal Engineer, but that of the small locks in England, and originated with the early English engineers.

J. P. FRIZELL, M. Am. Soc. E.—It appears to me that there is no subject in engineering to which so much ingenuity has been devoted as that of gates for controlling the passage of water.

I believe I understand the gate which Mr. Worthen has just de-

scribed. It consists of a cylinder standing over a circular opening. The latter is at the level of low water, the top of the cylinder is at the level of ordinary high water. The pressure of the water occasions no friction and offers no resistance to the raising of the gate. When the water rises above the top of the cylinder it operates as a waste gate with great efficiency, as it discharges under the full head due to the difference of level, provided that does not exceed the head due to the atmospheric pressure. This is an exceedingly simple form of gate, and susceptible of wider application than it has received. Gates upon this principle were used upon the old water-works of Paris nearly 100 years ago. I see no reason why such a gate could not be applied to an ordinary service reservoir, serving, at once, as a waste gate and an overflow. Usually, in studying any particular application of this gate, it will be found inconsistent with other conditions of the structure.

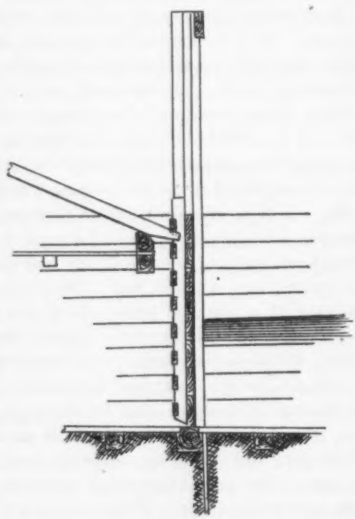
It is in very rare cases that gates running on wheels are to be recommended. It is usually better to apply the power necessary to move the gates, than to complicate their construction by arrangements for diminishing friction. In the head-gates of a canal, simplicity is more important than economy of working. Every additional part in such gates is an additional chance of derangement and possible delay. If the conditions imperatively called for devices of this sort, rollers, such as are contemplated at the power-house now under construction at Niagara Falls, would, to my mind, offer a more promising solution of the problem than large wheels. Again, if I adopted wheels, I should see no reason for departing from arrangements now in use at Lowell, Mass., which are said to have worked well. Mr. C. J. Cheney, of that place, is the inventor of a wheeled gate, which has been applied there in several cases. The peculiarity of this gate is that the wheel runs upon at rack or bar, which is cam-shaped in cross-section. When the gate is at rest, the wheels do not bear upon this track bar. The gate is held against its closing surfaces or seat by the pressure of the water. When the gate is to be raised, the cam-bar is turned through a quarter or half a revolution, and comes into bearing with the wheel, lifting the gate off its seat. The gate then runs freely on its track, which sustains the pressure of the water. When the gate is closed, the cam-shaped track bars are revolved out of contact with the wheels, and the gate rests firmly against its seat.

The hydraulic lift is an admirable arrangement for handling heavy gates. It is simple, free from complication and liability to derangement, not inordinately expensive, and where there is a domestic water supply, the power is always at hand. A force pump, to be operated by hand, is a sufficient safeguard in case of the failure of the mains. The cost of operation is trifling. For the head-gates of a water-power canal, which usually are not closed more than once a week, it is permissible to allow the gate to slide upon its seat or closing surface, but

for the wicket gates of large locks, which may be opened and closed many times a day, this is not desirable. Especially is this true of a wooden gate, or one with a closing surface of brass. To meet this condition, the simplest way is to make the sliding surface vertical, and the closing surface inclined. Then the pressure upon the gate does not have to be borne while moving by the closing surface, and the latter does not wear.

As to ice, it does not appear to me that there need be any difficulty in that, because you can always have your gates housed. Anchor ice only forms when you have open water above your bulkhead. So soon as the open water is closed by ice, you will have no further trouble from anchor ice.

The simplest and most convenient form of gate for depths of water not exceeding 5 ft. is made by lumbermen, and used for log and lumber sluices (see accompanying sketch). The gate consists of two uprights, sustaining the planking which forms the face of the gate. On the rear of the same uprights are bolted stout cross-pieces with intervals of some 6 ins. between them. The only lifting gear required is a couple of stout levers. The workman inserts the end of the lever under one of the cross-pieces, and weighs down on it across a block. The gate is lifted 3 or 4 ft., if necessary, above the surface of the water, and there fastened by a chock placed under one of the cross-pieces. To drop the gate it is only necessary to knock out this chock. The gate passes from the surface to the sill in a fraction of a second, so that the water has no time to drop away on the down-stream side, and so bring a pressure to bear on the gate while descending.



Mr. Herschel.—From the way the very eminent engineers who have spoken have received the remarks that have been made, I am inclined to think that there is a form of ice which has not been in their minds, and it is a very important form of ice to contend with. I say nothing about anchor ice, and do not wish at this time to take part in a discussion of that subject. You have all seen in a very

severe winter the handle of a door-latch covered with ice; it comes from the air, I suppose. The moisture is taken out of the air and is frozen. A similar but harder kind of ice than that forms below water. Take a corner filled by machinery, and in the corner there is still water. It is very cold weather, and by conduction away of heat from the metal surfaces the ice is formed. That is what I propose to speak of now. There are times when every little corner of a gate rigging will freeze up, or get coated with hard ice, and as far as one can see below the water, certainly some distance below the surface of the water, it is all one mass of solid ice. The gate will be frozen fast above the water, and above the surface of the water the ice shows in the shape that I have referred to just now; it looks like hoar frost all over the metal surfaces. The ice below water is very hard; it fastens everything together. I suppose it depends for its formation on little quiet places in the machinery. People that work with such machinery year in and year out have a way of looking at this matter entirely different from what we would be apt to get from looking at a drawing.

The gentleman who preceded me says you can house your gate; so you can, and you can also heat the house, and it is done as a general thing in New England; but that will not prevent the formation of ice below the surface of the water, nor even below the floor level.

Mr. Worthen.—I have had a little experience with ice. Ice forms in acicular crystals, which shoot across the surface of the water, and, if the water is still, the crystals come together to form a solid sheet; but if there is a ripple on the water, the crystals are broken up, mix with the water in the form of slush and attach readily to rock, iron or other material with which they are brought into contact. I have noticed a like breaking up of the crystals of saltpetre in the process of making gunpowder.

For many years I lived within, say, a quarter of a mile of the lower falls at Lowell, and I could always tell when the anchor ice was forming. As the anchor ice formed, the noise of running water was stopped, and I observed the condition of the air at that time, sharp cold, with a light breeze. If the mill pond froze over and kept frozen, there was no trouble with anchor ice.

Mr. FitzGerald.—About 18 years ago I lost a Christmas party from anchor ice, and, naturally, I have never forgotten that night spent in fighting this enemy of water supplies. Soon afterwards I began to study the question, and found it as Mr. Worthen says. The water in a reservoir begins to cool down in the winter, and if there are heavy winds it may not freeze over until after the whole body of water is cooled to the freezing point. This generally occurs at Christmas time with us. I now keep the run of the temperatures of the water, and if I find that the whole mass of water is in this condition and the surface

not frozen I keep men in the gate-houses to watch for anchor ice. It will form in a very short time after the conditions that I have described have been reached.

The anchor ice that I have observed is in the form of thin scales, about the size of a half dollar and needles. It comes in with the current, apparently from all parts of the body of water, and it has a way of collecting in punky masses, which soon fill up all openings. In my principal gate-house I have a steam pipe laid reaching into the water in front of the screens, so that I can quickly blow steam down into the masses of anchor ice and break them up.

J. F. O'Rourke, M. Am. Soc. C. E.—I want to say a little about anchor ice. I think the definition of anchor ice is one that is, perhaps, not understood in the same way by everybody. Our idea of anchor ice was that it, as Mr. Worthen describes, was ice that formed as slush, and finally became solid by running together. The fact that ice will form under ice from clear water to a considerable depth is one that came under my observation when we were building the Wisconsin River Bridge. It was about 20° below zero, and we put in pile foundations for piers. Those foundations were driven, and, some time afterwards when convenient, were cut off. There was 2 ft. of ice over the river at the time, and when the piles were sawed off, perhaps 15 ft. below the surface, the butts were covered with solid ice all the way from top to bottom. The only explanation we could give was that the water was in the condition Mr. Herschel describes; it was just at that point when it would give up the latent heat. Something that would remove enough of the latent heat to convert it into ice was all that was necessary. We accounted for the formation of this ice by the fact that these piles were exceedingly cold when driven, and that the frost penetrated down through the piles. I think the same thing is true of these gates. The top of the gate is out of water; it becomes much colder than the water can become without freezing, and contact with it freezes the latter far down below the surrounding ice that is covering the surface of the water.

Mr. Herschel.—The gentleman has described what I have attempted to describe to the meeting. I know, I think, a little about anchor ice, but I have not wished to discuss it to-day. The kind of ice that I think is the worst is what I wished to describe.

Mr. Keefer.—I would like to ask if the piles were standing in open water, and slush ice above and below?

Mr. O'Rourke.—It was very rapid water; the pier was at the side of an island in the channel.

Mr. Keefer.—Then there was open water above?

Mr. O'Rourke.—No, sir.

Mr. Keefer.—Was there water uncovered by ice anywhere above where the piles stood?

Mr. O'Rourke.—No, sir; the water was not all over the piles.

Mr. Worthen.—How far up?

Mr. O'Rourke.—There was perhaps 3 ft. of ice all over the river.

Mr. Worthen.—How far up was it frozen over?

Mr. O'Rourke.—It was frozen over all the way up as far as you could see.

Mr. Keefer.—Then there was no open water above that point?

Mr. O'Rourke.—Only such as was caused by the progress of the work. There was no opportunity for slush ice to form.

Mr. Keefer.—My reason for asking the question is that we find anchor ice will run 10 or 12 miles, more or less, according to strength of current, under the surface ice. That is a common occurrence in the St. Lawrence; in fact, at the beginning of winter it passes through the whole length of the river before that is frozen over. At the ice bridge, which first forms below Montreal, the floating anchor ice is carried under the surface of the bridge ice and accumulates until it hangs down from the under side like a gigantic wasp's nest; 50 ft. down, in some cases, where the water is deep enough. I have found it to extend 90 ft. below the surface at the head of Lake St. Louis and foot of a long rapid.

With regard to the formation of anchor ice, I think it has been correctly stated by Mr. Worthen and others, but as to its starting on the bottom or growing on the bottom I believe the explanation to be this: We find that at the first the whole surface of the St. Lawrence River is covered with the slush ice, which are needles of crystallization broken up by wind and current and drawn together by mutual attraction into little bodies and masses, and sometimes you will see the surface of the St. Lawrence covered with slush ice. That passes down until the water gets cold enough to bridge the river over, and where a lake or the tide meets it a bridge is formed, and the descending anchor ice gets directly under, and it glides along on the other side until its velocity is sufficiently retarded by friction to permit it to stop, when it freezes to the under side of the ice and grows downwards. When the descending ice has been elbowed by the current into all the side channels, confining the main current into one central part that carries this ice so much farther down until arrested by the surface ice, when it grows downward, forming a suspended dam, and then the river begins to rise and backs up until the winter level is 10 or 15 ft. above the summer one, and the wharves of Montreal are covered.

As you all know, and as Mr. Francis demonstrated, the surface water in a strong current or rapid descends to the bottom and rises again all the time; this not only reduces, as I believe, the temperature of the water at the bottom to the freezing point, but decidedly below it, and what only prevents the water from freezing solid is its motion; if it rested, it would freeze at once. The result of that is that a cur-

rent of water colder than the freezing point passes over the stony bottom which causes a rapid radiation and reduces the temperature of the bottom (as would a cold wind the human body) to such a point that the ice needles in the water take hold of the bottom, or the bottom takes hold of them, and "anchored" ice grows rapidly. I have known it to cover the whole bottom of the St. Lawrence, which, in open water, it covered to a depth of 2 ft. before it stopped growing. I have gone out on the river and put a pole down and pried up some of it, and it would come up with a rush and then float down. I have gone out when the thermometer was 20° below zero and I have looked out over the surface of the river and it was as clear as the Niagara—no ice on it; there had been several days of below-zero weather, and then suddenly the thermometer rose to 40°, the ice let go from the bottom and the whole surface would be dotted with white caps. They would burst up from the bottom, drain out at the top and settle down with 90% of depth below surface and sail away down. I have no doubt that our ice floods in Montreal are entirely due to anchor ice. Bridge ice, surface ice of any kind, would never sink and could not cause a dam, because these floods take place when the flow of the river is a minimum, there being no flow at all from the surface, and none but the very large streams bringing in much water. I believe, therefore, that the whole of our enormous winter floods at Montreal is due entirely to anchor ice formed in this way. It should be explained that the ice bridge is formed of floating ice, tilted by the current and standing on edge and exceedingly rough on the under side, just suited to catch and hold the anchor or slush ice which only goes under.

Arthur De Witt Foote, M. Am. Soc. C. E. (by letter).—The abstract of the very interesting description of these large head-gates built by Mr. Freeman reached me sometime after it was issued, but was of particular interest, as I was just finishing a design for a series of gates of almost precisely the same size and intended to bear about the same pressure.

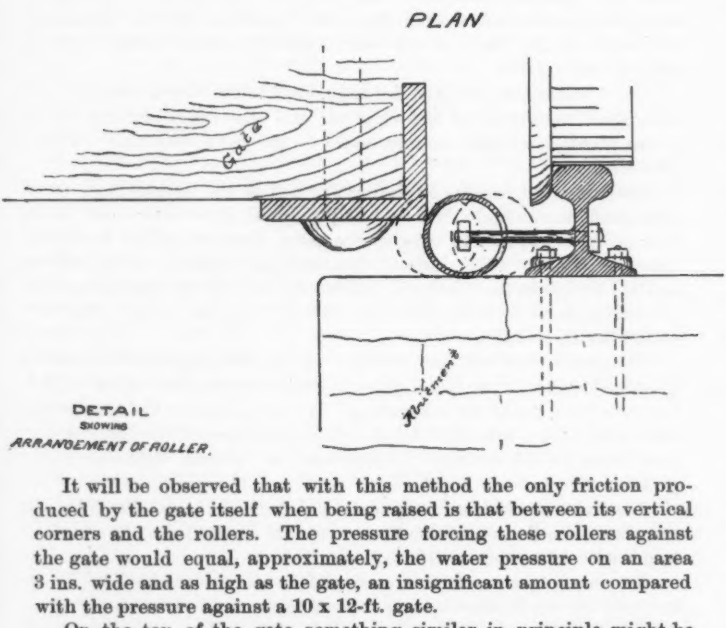
Some years ago I designed a large gate for use under a heavy pressure to run on wheels on a track like an inverted flat car, but in that case the gate was of steel and did not touch the abutments. In other words, the gate was held by the wheels so as to allow a ½-in. clearance and consequent leakage between its face and the structure. The leakage was no objection, except as it might draw drift into the crack and wedge the gate. Wishing to avoid this crack in my present work I was led to contrive something that would close it and still keep the gate from bearing on the abutments and consequently "sticking."

Here is where Mr. Freeman's method of having recesses for the wheels to drop into and thus bring the gate to a bearing appears to me to have serious objections. The gate not only will "stick," but the first few inches that the wheels travel on their way upward is up-

hill in reference to their load or pressure as well as in reference to gravity.

I have therefore kept the track for the wheels straight, carrying the gate so as to give 2 ins. clearance from the abutments, and, in between the tracks and the inner vertical corners of the gate, 3-in. pipes or rollers are fixed in such a way that they can roll backward and forward 3 or 4 ins., yet touching the abutment. The upper ends of these pipes or rollers are tapered so that the corners of the gate in descending will not strike them, but will wedge or roll them to one side and pass down between. The pressure of the water will hold these rollers in place so as to fill the crack or clearance space between the gate and the structure. The accompanying detail drawing will show the position of the rollers and method of holding them so that they can roll each way.

A 3-in. pipe is used simply because it is a cheap form of roller.



DETAIL
SHOWING
ARRANGEMENT OF ROLLER.

It will be observed that with this method the only friction produced by the gate itself when being raised is that between its vertical corners and the rollers. The pressure forcing these rollers against the gate would equal, approximately, the water pressure on an area 3 ins. wide and as high as the gate, an insignificant amount compared with the pressure against a 10 x 12-ft. gate.

On the top of the gate something similar in principle might be used, but for my present work I shall simply fit a piece on to work as close to the face of the structure as possible and expect a trifling leakage.

In the matter of a gate hoist I have adopted two stems with the ordinary form of ratchets and connected geared winches, one turn of the handle raising the gate $\frac{1}{4}$ in. I have arranged the cog wheels next the ratchets, however, so that each have two bearings, one on each side, close to the wheel, having found that where a bearing on one side only is used, the shaft is liable to spring under heavy strain sufficiently to break the cogs.

Of course, the idea embodied in the foregoing has not borne the test of experience, and it may seem ungracious to present it as an improvement on Mr. Freeman's design which is working, and working well I have no doubt; yet, as an idea it may be worth the recording, and if it prove a success Mr. Freeman will be the first to welcome it.

Robert A. Cummings, Assoc. M. Am. Soc. C. E. (by letter).—If the distinguished author of this paper deems it necessary to express his *motif* for communicating it to the Society, it would seem obligatory for a younger member to apologize. It is therefore with a considerate deference to the views of the author that the writer ventures on the floor of discussion.

The method here adopted for reducing the friction in raising canal head-gates has given rise to a vigorous oral discussion, during which it was pointed out that one man could do the work effectually by simpler means.

Realizing the enormous sliding friction of the contact surfaces of head-gates with a high river and empty canal, it is obvious that whatever power is applied in raising the gates, does not reduce their frictional resistance. The author, therefore, has used a rolling-friction method to reduce the frictional resistance, but with the multiplication of moving parts, bearings, etc., the writer would ask what is the estimated saving in friction?

The probability of dirt settling in the bearings and corroding them makes it an exceptional piece of construction, the future operation of which would be interesting. A comparison of the theoretical and actual power absorbed by the rolling friction of the gates would give some much-desired information on rolling friction under pressure.

The writer desires to record a method devised by him for reducing the sliding friction of head-gates, which it is thought may have a wide application in similar structures.

Returning to the author's method it is self-evident that the clearance of $\frac{1}{4}$ in. or $\frac{1}{2}$ in. from sliding contact is the main advantage secured where the rollers carry the load. This clearance of contact surfaces the writer accomplishes with a plain ordinary gate by introducing between the surfaces a film of water under pressure. The gate slides on the water only and is raised in the usual manner or by a hy-

draulic ram. The escape of the water is of inappreciable consequence. The pressure may be readily obtained from a hydrostatic hand pump, an elevated tank, or as circumstances dictate.

The formation of anchor ice, which is so common in the winter, must be cared for by a portable steam boiler and warm water used as pressure water. The temperature of the metallic portions of head-gates has a natural tendency to be uniform in a more or less degree, which may account for the formation of anchor ice below waterline.

James Francis, M. Am. Soc. C. E. (by letter).—The discussion of Mr. Freeman's design of the hoisting device for the head-gates at Sewall's Falls gives me the opportunity to describe briefly a form of gate which has been in use at Lowell during the past 10 years.

The proprietors of the locks and canals on Merrimack River have, at the present time, six gates at five different places, which can be moved from their seats by various methods, and the loads, due to the pressure of water against them, together with the weight of the gates, can be transferred to bearings, resting upon four to six wheels for each gate, and, after this transfer has been made, the gates can be operated up or down, like a car on a railroad track, with the rails in a vertical position.

The first appliance of wheels to water-gates, in Lowell, was made about 10 years ago, at the speed-gate of a turbine, where the water was admitted to the buckets through a wood scroll. The gate was 3½ ft. long by 2-ft. rise.

The very old hoisting arrangement there, consisting of a small worm and gear, in connection with a rack and pinion, had become worn out, and a new device, invented and afterwards patented by Cleveland J. Cheney, the Superintendent of Labor, was substituted. It was a crude apparatus, but it served the purpose of operating the gate quickly and easily, after the load was transferred to the four wheels.

Most of the other gates are placed in the wasteway dams, which are situated at various parts of the canal system, and they are operated under heads of water varying from 8 to 15 ft., or more, above the sills. The later designs are vastly superior to the first one which was built 10 years ago. These gates, which have a pressure of water against them varying from 2 to 15 tons, and sometimes more, can be readily and quickly operated by one man.

The first operation to be performed is to transfer the load from the seat of the gate to the wheels, which in the latest designs can be done by turning one-fourth round a vertical cam rod placed near, and parallel with, the end of the gate by means of a lever, and then turn-

ing another similar cam rod at the other end of the gate, which produces the effect desired, viz., the transfer of the load from the seat of the gate to the four or six wheels, which now bear upon the cam rods. The cam rods are made of steel, and they are secured in metallic boxes or journals at the sills and again near the top, and are made long enough to reach to a point above the surface of the water, where the operator can work the lever to the best advantage.

After this operation is completed, the gate can be readily raised, with the expenditure of but little power compared to the power that would be required without the wheels, by turning a crank which is connected by gearing with the vertical start or post which is attached to the gate.

The gate can be lowered with equal facility, and, after it is shut, the cam rods are turned back, and the gate is relieved from the wheel-bearings and comes to a firm seat against the slides, which prevents any further leakage around the sides.

The important feature of this design is the transfer of the load to the wheels before the gate is started.

Another feature regarded as essential is the counterpoise, which serves to balance the gate and to equalize the power of raising and lowering.

The writer regrets that he has not been able to prepare drawings of the gate machinery described within the time left to him since his attention has been called to this matter.

John R. Freeman, M. Am. Soc. C. E. (by letter).—I had hoped to present this paper in person, and, if then joining orally in the discussion, could, perhaps, have made some points more clear.

I am pleased that the design received such full discussion, and there is no part for which I am more grateful than for the frank criticism by my friend Mr. Herschel.

I understand Mr. Herschel's criticism on roller-bearings is directed more particularly toward cases where water-power or other power is available for hoisting the gate, not cases where hand-power must be used from reasons of first cost.

The intimations as to what may be feared from ice prompts the reply that I was not without thoughts of this when making the design, and can yet see no grounds for fear.

Ten years of winter and summer work on and around one of the chief water-powers of New England did not permit me to forget that water is not always fluid, or to forget that zero weather and ice, freshets and driftwood, control the factor of safety in structures of this class; neither can I, after having supervised repairs upon two or three bad washouts, in different parts of New England, forget that an important canal guard-gate should be capable of closing quickly

against an empty canal, with but one pair of hands for a prime mover.

Replying further, in order, to the various comments and criticisms, I submit that:

First.—One superiority of the rack and pinion gate-hoist over the screw and nut gate-hoist, for low-cost work and exposed situations, is that it is much cheaper. The rack and pinion is almost wholly foundry work, worth finished, say, 3 cents per pound, with a little carpenter work on the uprights; the other (say a 5-in. wrought-iron screw 14 ft. long) is machinist's work of an expensive class.

The rack-gear is more easily freed from ice than the screw, and, with the massive timber gate-start behind it, can better withstand a log-jam or ice-thrust than the more slender screw.

Gates lifted by screws should be placed within chambers, with a masonry wall carried over the up-stream ends of the sluice-piers, to serve for a fender and to house them from extreme cold. This again adds to the expense.

Second.—Doubts concerning a mechanism which runs down free, controlled only by a hand brake, may be lessened in view of the successful use of precisely this means of lowering at the North Canal head-gates, at Lawrence, Mass., twice each day for about 46 years. The quickness and ease with which a gate can thus be lowered, for about two-thirds of its height, is a point in its favor, and led me to dispense with the counterpoise.

Third.—The collection of gravel or rubbish against the rebate of the sill is provided for by a triangular block, which does not show clearly in the engraving. This forms an incline at about 30° just up stream from the face of sill, with the top of the slope about 3 ins. below the bottom of gate, and it is my hope and expectation that should any gravel or similar obstruction not scour through, that, when pressed by the descending gate, it will be pushed up stream down this slope enough to let the gate lap over the sill enough for a water-tight joint. Moreover, it is probable that gravel, sand or water-logged rubbish, rolling along down stream, will not fill up entirely flush with the up-stream edge of a sill where there is so swift a current, but will scour out a few inches below its up-stream edge.

One trouble that I have noticed in closing other large sluice-gates tight has come from water-logged brushwood getting caught under the gate; therefore in this design, and in one large gate previously designed, I thought I would try making the lower edge of the gate and the edge of its sill into a pair of mighty shears.

So far, the gate built in this way has proved very satisfactory, and the first one, just referred to, though often opened and shut, and with its leakage always in plain view, is remarkably tight without any

necessity for the "dusting" of ashes, shavings and the like, prescribed by Mr. Herschel for gates with a flush sill; and, by the way, as I have on perhaps a dozen or twenty occasions, passed some long and weary midnight hours waiting to enter a flume or wheel-pit, watching men bring coal ashes, gravel, picker dustings and burlaps by the barrelful and dump them in front of a gate with a flush sill. I know that a flat-sill gate can not "always be made water-tight by half a dozen shovelfuls of ashes," etc.

Fourth.—Roller wheels on head-gates would in many cases be unnecessary. At the head of an ordinary turbine penstock, or at the back gate of a wheel-pit, they would generally be an unnecessary complication, for such gates are seldom closed. The pressure on opposite sides can commonly be balanced while closing or raising, or in an emergency. At the time of a breakdown there are many willing hands around to furnish abundant power.

For a waste-gate to prevent an important canal or reservoir from overflowing, for the guard-gates at the head of a canal with easily destructible embankments, for a large regulating gate which must often be opened and shut without counterbalancing pressure on its downstream side, for important controlling gates off in the woods at the end of a telephone wire, I still believe such devices as roller-bearings to be of great value in enabling one man to exert the power of three men, or conversely, to do the work in one-third of the time possible with slow gearing.

The first gate that I built with roller-wheels was put in service about three years ago. It is over an opening of 28 sq. ft. area, under 10-ft. head on its center, and with no back pressure. The rollers can be thrown into play or out at pleasure. The master mechanic of the factory in whose yard this gate stands has lately been making some experiments on that gate by hanging weights on the hand cranks, placed horizontally, and finds that 33 lbs. will move the gate when the wheels are in play, and that 140 lbs. are needed when the wheels are thrown off.

This simple expedient of the wheels, therefore, enables 22½% of the power to do the same work, or one man to do the work with the rollers with the same ease as four men without the rollers.

This earlier gate was described by me in connection with an ice weir in a paper before the American Society of Mechanical Engineers (June, 1894, Vol. XV of *Transactions*), and the points therein developed bear upon this question.

This earlier structure, with its roller-bearings, has passed through two winters, one of them exceptionally severe. The gate is at the center of a wasteway for running off ice, and not the slightest defect or difficulty with any part of it has developed.

It stands guard against the overflowing of a canal 2 miles long, with

an easily destructible embankment of pure sand, and it was thought desirable to have a waste-gate which a night-watchman could raise quickly. The roller-wheels enable one man to do the work of four, as already stated.

Fifth.—Power other than manual strength for hoisting head-gates is a good thing when you can afford it, but out of the many large American water-powers, only Holyoke, Lowell, Bellows' Falls and Manchester are the ones which I recollect as having seen possessing it. Lawrence, Lewiston, Rumford Falls, Palmer's Falls, Berlin Falls, Nashua, Spokane and probably some scores of others of above 2 000 H. P. are without power hoists.

For the great majority of these the head-gates are worked but seldom, and then under circumstances when it does not matter much whether one man or six is detailed for this short job.

To illustrate the conditions governing in many cases, here at Sewall's Falls to put a turbine with accessories and connections for working the gates would have added probably at least $3 000 to the cost, and perhaps more, and it would have stood idle months at a time.

To multiply the gearing so that one man could work it comfortably would make the time of hoisting or of shutting in an emergency intolerably slow. Now, what did these roller-wheels with their trunions and special railway add to the cost? The four for each gate cost, altogether, about $185. I think they are worth this.

Be it noted, so little do they add to or complicate or modify the structure, the hoisting rigging and the gate being complete without them, they could be taken off in a few hours, and without adding anything whatever in their place, there would be left an excellent example of a gate fully equipped for being pulled up by four men to a crank in the good old-fashioned way, for all the rigging above is made as strong as though the roller bearings did not exist.

Sixth.—I don't see how these roller trucks are likely to get injured by ice, for it will be noted they are extremely massive, but suppose the rollers and their tracks become encased in ice, how can their existence increase the chance of breakage? You then will have at the start merely four "sled runners" instead of four wheels, and a 20-ton pull available to start the load, and with an axe and a pry and an "arch-kettle" full of hot water, the chances are fair that the gate can be started at least as easily as in the days of yore.

A slow-speed arrangement of great power, such as Mr. Herschel recommended, is provided in the ratchet bar, forming a part of this outfit, and by the time the man has, under the powerful purchase of this device, pulled the gate up 2 or 3 ins., the ice lodged in the wheel trunions, or along the track, will probably crush and cease to be very much of an incumbrance.

For Mr. FitzGerald's contribution to this discussion, I must return special thanks, since the 20 years' successful use of his roller-wheel device strengthens my confidence in the success of the gate-rollers described in this paper. It is curious to see how closely the two independent designs run parallel in the manner of lifting the gate off its slides by wedge-like tracks. I had seen no such arrangement before.

Mr. Hiram F. Mills, C. E., at about the same time that these Sewall's Falls gates were built, was introducing a very similar device of his own design at the canal head-gates on the large water-power at Winslow, Me.

The device which Mr. Fanning has contributed to this discussion, with its remarkable low cost and its eight years' record of successful use, appears to have a special field of usefulness.

After learning also, of the ingenious device of Mr. Foote, in connection with large gates carried upon rollers, I can but feel much pleased to find that my use of roller bearings has brought me into such excellent company, and, when my opportunity comes to try again, I shall surely profit by this present discussion.

Again, if trying, I should consider the avoiding of the endless screw arrangement, and using a secondary gear or two to gain the same speed reduction and gain in force, with, if possible, less loss in friction than involved by the best of worm gearing.

In experimenting recently at Sewall's Falls, with this apparatus at the top (which has not been properly protected from the weather, or well oiled, and has, therefore, become quite rusty), I find that the sharp angle of advance of the worm screw causes it to bite against the cast surface of the worm wheel with rather too much friction. This action being more noticeable than in an earlier design where cut worm gears were used.

Mr. Wm. E. Worthen has brought forward another style of gate which certainly has much to commend it for certain situations in furnishing at once a waste-weir and a balanced gate. I have had some experience with these, but had supposed them indigenous to about the latitude and longitude of Southern New Hampshire, and that they were the invention of Daniel Hussey, years ago a noted millwright and cotton manufacturer. From one cause and another, the half dozen large gates of this type which I have happened to see have been displaced within 10 years past by gates of the vertical, flat, sliding type. It is but fair to say that none of these was planned with such stiff and rigid foundations as might have been used advantageously, and were less strong in their bearings and supports than would be designed by an engineer of Mr. Worthen's great experience.

The men who operated these old "tub-gates," as they were called,

were prone to concur in the view expressed by Mr. Herschel and wish them in Andalusia, Arizona or some other hot country, where ice never forms; still the type seems to possess marked advantages for a drain-gate and with only the drawback that, so far as my little experience with them goes, it is sometimes difficult to shut them tight. It certainly is a type which ought to survive and for quick, easy action, for perfect balancing of the water pressure, and for containing within itself an over-flow weir of very considerable crest length, it excels any type with which I am acquainted.

They appear better adapted for a gate to be kept shut than for a gate to be kept open, and where the friction loss in the water flowing through them must be kept small, they present the same disadvantages as a globe valve, the two sharp right-angle turns to the current. By reason of the space they occupy they are not well adapted to handle very large quantities of water, as, for instance, upward of 1 000 cu. ft. per second.

For canal guard-gates alongside a river, I should always distrust them by reason of the tortuous passage (like that through a globe valve), not permitting a drift log to pass.

On the question of anchor ice, it may not be amiss, since two of our members have called attention to the desirability of a special turbine for furnishing power to hoist the gates, to recall how anchor ice stopped the working of the gate-hoisting turbine at the Northern Canal in Lowell once many years ago. In the early morning just before starting time for the mills, the turbine speed-gate was raised, that the main sluice-gates might be opened. The turbine immediately plugged up with the interlacing needles of anchor ice and would not move. Thousands of operatives were waiting to begin their day's labor, and tradition says things were in a terrible state around that gate-house for two or three hours, but just as other means had been improvised, the sun got up so high over the adjacent walls and roofs that its rays fell in the water in front of the feeder to this turbine, when suddenly the turbine started up and jogged along as if nothing had ever been the matter.

To the very interesting remarks of our Past President, Mr. Keefer, I will add but one brief observation. Ten years ago I was making soundings in cold December weather at mid-channel in some rapids on the Merrimack River from a boat held by a hawser stretched between trees on the bank. The cold had not yet fully closed in the river above. On prodding the rocky bottom with my sounding rod, I found it almost everywhere covered, even in swift current, with irregular masses of interlaced needles of ice from 2 to 4 ins. in thickness. I pulled up many bunches with my boat-hook and in some more shallow places could see their spongelike masses plainly *in situ.*

The special peculiarity which I noticed was that the bunch was perhaps four-fifths water, and that the ice crystals ran in every direction like shingle nails in a keg. Thin fingers and patches of surface ice were running on top of the water, but their crystals were in the horizontal plane, while in this anchor ice the crystals were noticeably different.

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SOME NOTES ON HOT-BATH TESTS FOR CEMENTS.

By FREDERICK H. LEWIS and J. EDWARD WHITFIELD.

READ SEPTEMBER 5TH, 1894.

WITH DISCUSSION.

On December 1st, 1886, a paper on cement testing, written by Mr. Henry Faija, M. Inst. C. E., was read before the American Society of Civil Engineers. In this paper Mr. Faija advocated the use of an apparatus which he called a "steamer," and which he claimed would reveal any "blowey" tendency in cements. The test consisted in keeping pats of cement in vapor and in water at a temperature of about 110° Fahr.

At a meeting of the Society on May 18th, 1892, W. W. Maclay, M. Am. Soc. C. E., presented a paper on hot tests for cements, in which he advocated the use of an apparatus which was exactly similar to that of Mr. Faija, as a means of discovering free lime in cement. Mr. Maclay, however, proposed to use a temperature but little short of the boiling point, or from 190 to 200° Fahr.

Between the dates of these two papers, the entire subject of accelerated tests for cements, hot-water tests, oven tests, Bunsen burner

tests, etc., was much discussed abroad, chiefly by Messrs. Candlot and Le Chatelier, in France, and by Messrs. Michaelis, Erdmenger and Tetmajor, in Germany.

More recently, at the Engineering Congress at Chicago last year, were presented the very interesting papers on cements by Messrs. Faija and Gary in the civil engineering section, and by Mr. Le Chatelier, before the mining engineers, together with their discussions. Each of these papers had something to say on hot-water tests of cements. Mr. Faija, we find, is in exactly the same place he was in 1886, and advocates the use of his steamer at 115° Fahr. as a means of detecting a "blowey" cement, expressing confidence that this temperature gives more reliable results than the higher one suggested by others.

Mr. Le Chatelier advised the use of an apparatus to discover the constancy of volume of cement in hot water.

Mr. Gary gave in detail the standard methods of testing cements in Germany. All hot tests are discarded in the German standards, for the reason, Mr. Gary tells us, that they have been found misleading. The test for constancy of volume is the thin pat in water at ordinary temperatures, precisely the same as the test recommended by the Committee of the American Society in 1884.

In the consideration of these opinions, the German decision against hot tests in specifications must evidently carry great weight. Because, in the first place, the Germans have studied cements, both theoretically and practically, in the most thorough manner; and, in the second place, because the German cements, as regularly manufactured, almost invariably withstand hot tests. As their competitors in England and America are very much less successful in this regard, the German opinion appears to be without commercial bias.

The writers of this paper have kept a hot-water apparatus in use and under careful observation for rather more than a year past, and have made during this time, in addition to regular tests, many experiments developing some points of interest, which it is their intention to present in this paper.

The hot bath and steam chamber have been kept uniformly at 180° Fahr. in a great majority of the tests. This temperature has been maintained by a Bunsen burner, with a constant pressure valve on the supply pipe. Occasionally only has the 115° Fahr. of Mr. Faija been used.

At the outset, the hot tests are interesting and convincing. The contrast is sharp between those pats which come out sound, firm and metallic, and those which show different grades of deterioration, varying from a few cracks to complete disintegration and rottenness; and the favorable impression produced by the sound pats is almost irresistible. If, therefore, this test can be shown to be really significant, it is a very desirable one.

In a course of experiments with lime, however, one is soon led to question whether free lime is not something of a bug-a-boo. It is quite true, as shown by Messrs. Le Chatelier and Maclay, that the addition of moderate percentages of caustic lime will cause swelling in pats placed in hot-water baths. But if hard burned lime is ground to pass through a No. 74 sieve, it soon ceases to be caustic. In a few days it is quite dead, and hydrates with little change of temperature. The fact, of course, is that in a powdered condition it rapidly takes up water from the air. It is probable, therefore, that in a week or ten days after grinding the free lime contained in a cement has ceased to be caustic.

Now, thoroughly hydrated lime is of no disadvantage to a cement. On the contrary, the strongest sand mortars are made up with mixtures of well slaked lime and Portland cement (see Candlot, page 220 to 224). Thus, in experimental tests by the writers, with a mixture of two-thirds Portland cement and one-third slaked lime, a mortar of 2 parts sand and 1 part of the cement mixture showed in test briquettes a tensile strength of 423 lbs. in seven days, and 583 lbs. in 28 days. Pats of this mixture of cement and lime also stood boiling tests perfectly, whether made up with fresh water or with sea water. We have also Candlot's authority for the statement that mixtures of small percentages of lime with Portland cements are much used in Germany to make cements more plastic. He says (page 221, "Ciments et Chaux Hydrauliques"):

"In Germany the use of mixtures of lime and cement has had a great development. The addition of lime to the cement is made in quite small proportion, and it serves only to give to the mortar a sufficient plasticity to be used with facility. A mortar composed of 1 part of cement to 6 or 7 of sand is often lean, little bonded and works difficultly; in a great number of cases, the resistance of such a mortar would, however, be quite sufficient; by adding a small quantity of lime in powder or in paste, the mortar is given the quality it lacks and becomes easy to use."

It follows, then, from these facts, that we need not care very much about free lime, if only it is well slaked.

Now it is a well-known practice in German cement manufacture, to make cements slow setting at will by the addition of sulphate of lime ; and it proves to be a fact that when cements are thus rendered slow setting they will in most cases at once stand boiling, although they had failed to do so when the set was quick. This fact was first called to the writers' attention by Mr. Thomas D. Whitaker, the president of the Whitaker Cement Company, and has since been quite fully established. Several brands of English and American Portland cements which would not "boil" when received, readily did so when made slow setting. Even a cheap grade of natural cement from the Lehigh Valley, which came out of the bath perfectly rotten when first tested, was found to be firm, hard, and stuck to the glass after a suitable addition of sulphate. Another natural cement, which had been rendered slow setting by sprinkling with water when drawn from the kiln, also stood the hot-bath tests admirably.

Not only is it true that cements can frequently be made to stand boiling by being made slow setting, but they will carry through the hot test a considerable added percentage of lime. Thus an American Portland cement, which at first would not "boil" satisfactorily, gave the following results on seven-day tests of neat briquettes :

No. 2 335	432 lbs.
" 2 336	463 "
" 2 337	503 "
" 2 338	449 "
" 2 339	475 "
Average	464 lbs.

This cement was then rendered slow setting, and 10 per cent. of quicklime was added as dry powder, and two briquettes were made from the mixture. The lime was ordinary hard burnt lime bought from a dealer and freshly ground for the test. The briquettes swelled and developed cracks in the moulds, but, after setting for 24 hours, were put in the hot water (180° Fahr.) and kept in it for 48 hours. They were then tested with results as follows :

Experiment No. 95	460 lbs.
" " 96	500 "

These broken briquettes have been lying on a shelf ever since (over three months), and are hard and sound with no signs of disintegration, except the original cracks mentioned above.

Another brand of American Portland cement which was quick setting gave the following results on seven-day tests of neat briquettes:

No. 3 085	500 lbs.
" 3 086	483 "
" 3 087	462 "
" 3 088	482 "
" 3 089	512 "
Average	488 lbs.

When made slow setting, 3 per cent. of quicklime, which had been specially calcined in a crucible at the high temperature of a steel furnace, was added. The briquettes were hard and sound, and after 24 hours were put in hot water at 180° Fahr. and kept at that temperature for 48 hours. They were then tested with results as follows:

Experiment No. 150	559 lbs.
" " 151	504 "
" " 152	598 "

Another portion of this second cement was then mixed with 10 per cent. of this steel furnace lime, which had been ground up fine and kept in a box in the air for a week. The neat briquettes came out entirely sound, and three of them gave the following results on seven-day tests, one day in air, six days in water:

Experiment No. 168	506 lbs.
" " 169	441 "
" " 170	446 "

Three other briquettes were put in the hot water at the end of 24 hours (180° Fahr.), and were kept in it for 48 hours and then tested with results as follows:

Experiment No. 186	421 lbs.
" " 187	420 "
" " 189	416 "

These briquettes showed a few small cracks on top, but no other defects. They have now been on the shelf for over two months, without change.

These results show the remarkable capacity of slow-setting cements to carry free lime, and to carry it without injury as far as present evidence goes, and of course it is thus evident that hot-bath tests do not reveal the presence of free lime in such cements. It may be said in reply to this that the free lime which does no harm need not concern us, and that the hot test is still good for the free lime which is injurious. But this claim begs the question. In Mr. Rudolph Dyckerhoff's discussion of Mr. Maclay's paper in 1892, he says that the test in no manner corresponds with practice, as fresh cement mortar is never exposed to hot water, and, he adds, "when thoroughly hardened, the mortar of any good Portland cement will safely pass this violent test."

Taken in connection with the tests set forth above, this very clearly explains the German position. From familiarity with the difference in behavior of cements, after being made slow setting, they have evidently reasoned that a quick-setting cement will either be able to deal with its content of free lime in ordinary course, or else its failure to do so will be manifested in cold water after the lapse of a few days. It is difficult to escape this conclusion; if a cement will not stand boiling in a fresh pat because of free lime, but will stand boiling in a pat a few days old, then it must in the interval have disposed of its free lime. The French authority, Mr. Candlot, substantially agrees with this view. He says (page 144):

"A cement of normal composition, not containing lime in excess, but in making which the burning has not been pushed to complete vitrifaction, swells enormously when immersed in hot water. Meanwhile, we have seen that such cements give in ordinary tests very good results, entirely different from those which are obtained from cements which contain an excess of lime. * * * * The mortars placed in sea water show no sign of alteration, and the test samples preserved in air give entirely satisfactory results. The test of the pure cement in hot water would meanwhile condemn such cements as containing an excess of lime."

As a test of free lime, the high-temperature bath has not, then, commended itself to the writers. Whether Mr. Faija's test at 115° is more satisfactory or not, they are not able to report from lack of

sufficient data. Such tests as they have made at 115° have been uniformly satisfactory with Portland cements.

It has been clearly suggested by some of the tests that there are other chemical factors of disintegration in hot tests besides free lime. Some cements are quite disintegrated in the hot bath, and one or two have failed to be entirely satisfactory after they were made slow setting. Analyses have showed these samples to contain high percentages of the anhydrous sulphate of lime; that is, the sulphate which is burnt in with the cement.

It is well known that the French specifications limit this anhydrous sulphate, and in all manufactures of cement there is an endeavor to keep it out of the product, although the reasons for this are not very clearly explained in the standard literature.

While, then, the hydrous sulphate which is added to cement after burning is not at all injurious in small percentages, the presumption is that the anhydrous sulphate is, and, as a working hypothesis, the writers have assumed that very bad pats are due to anhydrous sulphate. So far, the facts have confirmed this hypothesis, and a further investigation may lead to interesting results.

DISCUSSION.

R. W. Lesley, Assoc. Am. Soc. C. E.—The paper that Mr. Lewis and Mr. Whitfield have presented contains probably the latest information that we have on boiling tests for cement, and corroborates Professor De Smedt's and my own experiments. There is another side of the subject that has not been touched, and that is the fact that a great deal of this matter relative to the addition of gypsum, and of slaked lime to cement, and also relative to making it slow setting by water, is the subject of patents that were taken out a great many years ago, and which were preceded by many interesting experiments. In 1880 a patent was taken out by an Englishman, whose name, I think, was Jackson, who had invented an addition to the ordinary cements, making what he called selinitic cements. This patent consisted in the addition of gypsum to our ordinary natural cements, to make them slow setting, and, as he claimed, stronger. At that time I spent a great deal of time in investigating the subject in connection with Prof. De Smedt, the well-known chemist. We found that, while gypsum would make some cements more adaptable for immediate use, it gave the cement a

deceptive strength and a slower setting. It was tried with the quick-setting cement of the Potomac region, and, while it made the short-time tests higher, it had the effect of lessening the strength at long periods, especially in sand mortars. For this reason the patent never came into general use in natural cement manufacture. One manufacturer used it for a time and then abandoned it. Since then, however, the addition of gypsum to Portland cement, with the effects stated by Mr. Lewis, has come into use.

Another patent taken out in 1883 by Prof. De Smedt related to the making of quick-setting cements slow setting. It consisted in drawing out from the kilns the burnt material and sprinkling it with water. The theory is that by this moistening any small excess of caustic lime would be hydrated, and the cement rendered safe and slow setting. Chemical calculation by analysis is recommended to ascertain the percentage of caustic lime, and the proper percentage of water to hydrate the same is to be sprinkled by the use of a hose with fine spraying attachment. This patent I understand has been lately used without right by a number of natural and Portland cement works.

A third process, also patented, is the addition of slaked lime. There were a great number of experiments with this, and it was found that by the addition of slaked lime up to 15% the ordinary natural cement, as well as the Portland, is much improved. One theory upon which those experiments were conducted was that the setting of many cements is produced by the alumina, and that by adding slaked lime, which acted quicker than the alumina, the set is retarded and the cement is made marketable more quickly. The slaked lime also acts as a flux, making the mortar more plastic.

For the more successful use of the process of dampening or moistening the clinker or burnt stone, in 1884 I took out a patent for mechanically accomplishing the purpose by injecting steam or moistened air in the conveyor or elevator boxes. So much for the patents on this subject.

The difficulty about the boiling test that has always struck me is that no set of men, or no two men, can ever agree upon the exact method of this test. There is absolutely no agreement as to exposure, temperature or methods of testing. Since Mr. Maclay's paper and the reply to it which I made some three years ago, we have had the papers at the Chicago Exposition of Faija and Gary, both against the boiling test. It was also there that so eminent an authority as Mr. Whittemore said that the boiling test was of little value in determining the ultimate quality of cement. The German Society had this question up at their last meeting, and again adhered to the cold-water test. There Dr. Pruessing took up all the tests of heat and boiling, and, after recommending a modified boiling test of his own, stated that he had another test to recommend to determine the fresh

grinding of cement. This is to be done by placing a pat of fresh-ground cement mortar on an iron plate, heated to 392° Fahr. Then cold water is carefully poured on top until the pat is wet through and through, when it is again heated to 392° Fahr. Dr. Pruessing had the grace at least to admit that "in the test, by careless manipulation, the cake may tear without the cement being a checking one." No one who has ever as a child tried to bake mud pies will deny the accuracy of this last statement. In illustration of this, in a long series of experiments conducted in 1883 and 1884 in connection with the De Smedt-Willcox-Lesley patents for making Portland cement by dispensing with the intermediate process of brick making and drying of the slurry, it was found in every case where wet paste or wet brick were subjected to immediate heat a crust formed on the outside, steam or vapor formed on the inside, and that whenever this vapor forced its way to the surface it broke and destroyed the harder surface, and that a new surface formed as the process continued. This was overcome by mixing with the cement slurry, liquid hydro-carbons, which, by burning from the outside to the interior, formed pores, through which the interior moisture found a vent to the surface without destroying the latter. This process, improved by the adoption of heavy compressing rolls to make the slurry into egg-shaped balls well adapted to handling, was used for many years, and only abandoned when the introduction of water gas made the price of the coal-pitch combustible used in the mixture prohibitory.

Dr. Pruessing refers to the many accelerated tests which have gone before; the kiln test abandoned in England, and revised by Shepky; the glass test of Michaelis, in use 33 years, and rejected in 1876 by its inventor. He also refers to Heintzle's unsuccessful modification of this test, to Deval's hot-water test and Schumann's attack on it, and all other accelerated tests. He speaks adversely of the kiln and ball tests, which he says are "too uncertain," and then gives in table form results of some experiments of his own on 24 Portland cements, for the purpose of showing that his own method of testing is right and should be adopted. This method of testing and its results I shall refer to later, as, in my opinion, they do not prove what is claimed for them, but do prove that the ordinary cold-water test, in use here and in Germany, will, so far as change of volume is concerned, detect any imperfect cement.

The point that I am endeavoring to make is that, with these constant changes in these various grades of accelerated tests, it is difficult for the manufacturer, consumer or engineer to know just what is right and just what should be the standard. When it is considered that there are some 20 000 000 barrels of Portland cement made in every year, involving nearly $30 000 000, and that the great bulk of it is subjected to tests, it naturally becomes a very serious subject as to

what these tests shall be, and it is a very serious matter to take all this cement and subject it, not to well-defined, well-established and well-governed tests, but to a test which varies with the various differences of opinion between various men of various mental constitutions as to their opinion of temperatures, time of exposure and methods of testing. In view of all these differences of opinion and all these sudden changes that have taken place in accelerated testing for the last 25 years, the natural effect has been created that there is constantly a new set of tests coming out, and we must "wait for the latest," and when the "latest" comes, there is still the feeling that there is something to be later and more fashionable. For this reason, I again repeat, that in the face of the established tests in Germany, France, England and this country, none of which contain any of the accelerated tests, it is hardly a wise thing to make any change at this time; and several illustrations of the importance of this particular point have lately come to my attention, one of them in the city of New York.

On a piece of public work, a certain brand of cement is being regularly tested by the boiling test, and, as I am informed, has stood this test. Cement of precisely the same brand, made by the same works, shipped at and about the same time, was sent to two other large pieces of work in this same city. On both of these latter pieces of work, the ordinary cold-water test for checking of the American Society of Civil Engineers was in use. In both cases the cement failed, showing checking and cracking to a considerable extent. Certainly there is in this state of facts quite a commentary upon the effect of the boiling test.

In another experience, the manufacturer was informed by an engineer that the latter was going to boil some of his cement. The manufacturer quickly, for his own satisfaction, sampled the same barrel for the purpose of making in his own office similar tests. Within a few hours, he was at a leading testing laboratory with a serious expression on his countenance, and wanted a sample of the cement boiled at once, stating he had just been experimenting with this cement in his office, and had boiled it all to pieces. The testing laboratory took the cement and after two days of boiling in all the various methods known, were unable to make it check or crack. Differences such as this are occurring in daily practice.

In a third case, a cement which was largely used on an important work at Niagara, and which in most cases stands the boiling and yet is not manufactured with sulphate of lime with that end alone in view, had possibly the best actual boiling test within my knowledge. One of the plates over the firebox of a small movable boiler used by the contractor showed a crack. There was no time to repair it, and neat Portland cement was plastered over the crack. After it dried, a fire

was made, and for several days until the boiler-maker came, the cement stood the fire beneath and the boiling water above.

A further illustration is noted by Rudolph Dyckerhoff, Proceedings German Cement-Makers' Association, February 23d and 24th, 1894. He said, in commenting upon the accelerated tests then proposed for examination by the Society:

"When the boiling test first became known, I made, some 10 years ago, the following experiments with cements which had failed in the boiling tests and had stood the normal (cold-water) test. First, two pats were made from two cements. Of these, one pat after 24 hours' hardening in water and the other after 28 days' hardening in water, were duly boiled. The pats cooked after 24 hours' exposure were checked, but the 28-day pats remained good. Later on a great number of pats were prepared from various cements which had failed to stand the boiling test, and boiled after 24 hours, one, two, three and four weeks' hardening in water. After 24 hours' hardening, none of the cements stood the boiling. The longer, however, the pats were hardened in water, the better they stood the boiling water. Some stood it after two, others after four, weeks At periods over 28 days' hardening naturally, the pats of these cements stood the boiling. In later years I have repeatedly made similar experiments with various cements and received very similar results. I have also observed that if a cement is very strongly attacked by boiling after 24 hours' hardening, it requires more than four weeks' hardening to stand the boiling test. For instance, I received last year from London a sample of Portland cement which was ground very coarsely. A cake made of this cement was boiled after 24 hours' hardening, and checked, but it stood the boiling better each time after it had hardened one, two and three weeks, but even then the cake four weeks old was still full of cracks. Unfortunately, the quantity of cement in my possession was not sufficient for a larger number of cakes, but I am firmly convinced that, after a few weeks more, the cement would have stood perfectly the hot test. It may be mentioned that this cement as well as all the others I have discussed had stood the normal test. It seems clear that if a cement has stood the normal test, and does not stand the hot test at the stage of hardening, after 24 hours, but does stand this test after a longer period of hardening, such cement cannot contain any injurious lime; otherwise it would not stand the hot test ever so little at the later period."

Referring to the table of Dr. Pruessing, to which I have alluded above, his figures have an interesting bearing upon that portion of Mr. Lewis' paper, which is also substantiated by Dyckerhoff, and which refers to the fact that if a cement has not enough excess lime in it to cause swelling in the cold-water test, it will take care of any excess lime that it may have in it. The table in question was constructed for the purpose of demonstrating the value of the new accelerated tests of Dr. Pruessing. These tests he describes as follows:

"I take 100 drams of cement and from 5 to 7 parts of water, the amount of water depending upon the degrees of fineness of the grinding of the cement. The material is mixed until the cement has been thor-

oughly moistened by the water. It is then pressed into a cylinder mould and with the assistance of a power press is pressed into a round, sharp-edged cake."

In the table given herewith, the Pruessing tests are headed A, B, C, D, and are as follows:

A.—Pat 24 hours hardened in air, and 27 days hardened in water.

B.—Pat quickly put on an iron plate, heated to 392° Fahr., then wet throughout, and again heated to 392° Fahr.

C.—Pat after 24 hours' hardening in the air, then being placed for 2 hours in cold water, and then left for 24 hours at 194° Fahr.

D.—Pat 24 hours' hardening in air, 2 hours exposed to cold water, 27 days' hardening in water at 394° Fahr.

This table also contains practically all the accelerated tests that are well known, and also has a column devoted to the ordinary cold-water normal test of cement. As presented to the German Society, it also contains full records of the strength of the cement neat and in mortar, by tensile and compression strain, which are, however, omitted in the brief condensation of the table presented herewith, and bearing on the accelerated tests only.

By an examination of these tests, it will be seen that the one cement that shows the highest analysis in lime, to wit, No. 13, checked in the normal or cold-water test, as it also checked neat in nearly all the other tests to which it was subjected. It stood, however, in 3 to 1 mortar, the Erdmenger steam-pressure test, and neat it stood the Bunsen burner ball test. It is low in sulphuric acid, the slow setting being due to the excess of lime. It may be further noted that the cements which stood most of the accelerated tests were very slow setting, taking from 210 to 840 minutes to set, while those that failed to stand the accelerated tests were cements that took 8, 4, 300, 6, 70, 120, 35, 60, 10 and 840 minutes to set. It will also be noticed that those cements which stood the accelerated tests with the least advantage were those that showed (with only one or two exceptions) the coarsest grinding on sieves of 900 and 5 000 mesh per square centimeter. By a reference to the analyses of the various cements, it may be noted that in every case, with two exceptions, the cements which stood the Pruessing accelerated tests contained the larger percentages of sulphur as well as very much larger percentages of lime, thus showing addition of sulphate of lime. For purposes of comparison, I have tabulated herewith (see page 335), under the respective heads of "Stood" and "Failed," the various cements which stood or failed to stand the Pruessing accelerated tests, giving them by numbers and appending to each the percentages of lime, and sulphur, together with the time

	Fineness Residue on Sieve.		Percentage Water from Mortar.	Time of Setting. Min's.	Result German Normal Test.	Of the Tests for Permanence of Volume.					
	900 M per sq. cm.	5 000 M. per sq. cm.				Kiln Test.	Ball Test.	Steam Pressure Tests.	Pruessing Test as Des		
									A.	B.	C
I..	1%	24%	29%	210	Good.	Good.	Good.	Good.	Good.	Good.	Go
II..	1%	18%	28%	210	Good.	Good.	Good.	Good.	Good.	Good.	Go
III..	1.5%	13%	28%	360	Good.	Good.	Good.	Good.	Good.	Good.	Go
IV..	6%	26%	30%	8	Good.	Good.	Small check	Good.	Bent, small check.	Checked.	Chec
V..	7%	33%	40%	4	Good.	Good.	Good.	Good.	Checked.	Checked.	Chec
VI..	2%	19.5%	29%	195	Good.	Good.	Good.	Check free, but soft.	Good.	Good.	Go
VII..	1%	16%	29%	540	Good.	Good.	Good.	Check free, but soft.	Good.	Good.	Go
VIII..	4%	22.5%	30%	480	Good.	Good.	Good.	Check free, but soft.	Good.	Good.	Go
IX..	8%	30%	30%	6	Good.	Good.	Good.	Check free, but soft.	Checked.	Checked.	Badly c
X..	1.5%	23%	29%	70	Good.	Checked.	Good.	Good.	Checked.	Checked little.	Chec
XI..	3.5%	23%	30%	15	Good.	Checked.	Good.	Good.	Good.	Good.	G
XII..	8.5%	38%	30%	300	Good.	Good.	Good.	Check free, but soft.	Good.	Good.	G
XIII..	0.5%	16%	30%	120	Checked.	Checked.	Good.	Check free, but soft.	Check free, but soft.	Check free, but soft.	Chec but
XIV..	3%	23%	30%	35	Good.	Good.	Good.	Checked.	Good.	Checked.	Badly
XV..	6%	27%	30%	840	Good.	Good.	Good.	Check free, but soft.	Good.	Good.	G
XVI..	12%	36%	28%	840	Good.	Good.	Good.	Very badly checked.	Checked.	Good.	Badly
XVII..	7.5%	39%	27.5%	60	Good.	Good.	Good.	Very badly checked.	Badly checked.	Checked.	Begin disi
XVIII..	7%	32.5%	30%	10	Good.	Good.	Good.	Check free, but soft.	Checked.	Good.	Badly
XIX..	2%	27%	30%	540	Good.	Good.	Good.	Good.	Good.	Good.	G
XX..	2%	28%	30%	600	Good.	Good.	Good.	Good.	Good.	Good.	G
XXI..	5%	20%	30%	345	Good.	Good.	Good.	Good.	Good.	Good.	G
XXII..	5.5%	35%	30%	660	Good.	Good.	Good.	Badly checked.	Good.	Good.	G
XXIII..	4%	33%	30%	650	Good.	Good.	Bent, small check	Good.	Good.	Good.	G
XXIV..	2%	30%	30%	750	Good.	Good.	Good.	Badly checked.	Good.	Small check.	Ver ch

* I. Not enough cement for these tests.
† XXI. Cement with large addition of furnace slag.
‡ XXII. Cement with small amount of furnace slag.

Tests for Permanence of Volume.				Erdmen
Pruessing Test as Described.				Perms
A.	B.	C.	D.	Pure Ce
Good.	Good.	Good.	Good.	*
Good.	Good.	Good.	Good.	Goo
Good.	Good.	Good.	Good.	Goo
Bent, small check.	Checked.	Checked.	Checked.	Goo
Checked.	Checked.	Checked.	Checked.	Goo
Good.	Good.	Good.	Good.	Check but s
Good.	Good.	Good.	Good.	Check but s
Good.	Good.	Good.	Good.	Check but s
Checked.	Checked.	Badly checked.	Badly checked.	Goo
Checked.	Checked little.	Checked.	Badly checked.	Goo
Good.	Good.	Good.	Good.	Check but wha
Good.	Good.	Good.	Small check.	Check but wha
Check free, but soft.	Check free, but soft.	Check free, but soft.	Check free, but soft.	Check but wha
Good.	Checked.	Badly checked.	Badly checked	Chec
Good.	Good.	Good.	Good.	Check but
Checked.	Good.	Badly checked.	Very badly checked.	Bac chec
adly checked.	Checked.	Beginning to disintegrate.	Disintegrating.	Check but
Checked.	Good.	Badly checked.	Disintegrating.	Ba chec
Good.	Good.	Good.	Good.	Go
Good.	Good.	Good.	Good.	Go
Good.	Good.	Good.	Good.	Go
Good.	Good.	Good.	Good.	Ba che
Good.	Good.	Good.	Good.	Go
Good.	Small check.	Very badly checked.	Bent, small check.	B che

rdmenger Steam Pressure Apparatus.		Analysis of Cement.		
Permanence of Volume.				
ure Cement.	3:1 Mortar.	Ca O.	Mg O.	SO_3.
*	*	64.80	0.95	0.22*
Good.	Good.	64.48	Trace.	1.03
Good.	Good.	63.60	1.01	1.64
Good.	Good.	62.80	0.95	0.49
Good.	Good.	62.88	Trace.	0.49
Check free, but soft.	Good.	64.08	Trace.	1.47
Check free, but soft.	Good.	63.60	Trace.	1.17
Check free, but soft.	Good.	63.88	Trace.	1.19
Good.	Good.	64.30	Trace.	0.49
Good.	Good.	61.20	1.64	1.31
Check free, but somewhat soft.	Good.	61.10	1.27	0.63
Check free, but somewhat soft.	Good.	62.96	2.05	1.10
Check free, but somewhat soft.	Good.	65.20	1.74	0.67
Checked.	Checked.	63.60	1.79	0.63
Check free, but soft.	Checked.	60.96	Not determined.	1.24
Badly checked.	Checked.	60.56	2.11	0.42
Check free, but soft.	Checked.	59.60	2.31	Not determined.
Badly checked.	Good.	59.52	2.33	Not determined.
Good.	Good.	64.56	1.03	Not determined.
Good.	Good.			
Good.	Good.	50.56	1.27	1.28†
Badly checked.	Good.	59.74	2.68	0.88‡
Good.	Good.	64.18	1.40	0.56
Badly checked.	Badly checked.	61.18	5.70	0.42

of setting. It will be noted that the average time of setting of those which stood these accelerated tests was about seven hours, while the average time of those which failed to stand the accelerated test was two hours; that the average lime of those which stood the accelerated test was 63.52%, while the average lime of those cements which did not stand the accelerated test was 61.93%, and that the average amount of sulphur in those cements in the table "Stood" was 1.01%, while the average amount of sulphur under the head "Failed" was 0.70%.

In the making of averages of this table, where the element of sulphur had not been calculated, as in Nos. 17, 18, 19 and 20, the figures of these samples are not taken into consideration in arriving at the averages of sulphur, and the same applies to No. 20, as to lime, where there was no analysis at all. No. 21, which is "largely adulterated with furnace slag"; No. 22, "small amount of furnace slag," and No. 24, with nearly 6% magnesia, are also excluded as abnormal cements. No. 13, which failed in the normal test, is also excluded. The conclusion, therefore, that I have arrived at from these tables is, that any Portland cement, even though it contains lime in considerable excess of what it should contain, to make the proper combination with the alumina and silica, may be made to stand the Pruessing, or in fact practically nearly any of the accelerated tests, by the addition of sufficient sulphate of lime mechanically ground with it. This conclusion is deduced from the figures given, showing that in nearly all the cements that stood these Pruessing accelerated tests, the lime, or lime and magnesia combined, were very high, the sulphur high and the time of setting slow. On the other hand, in nearly all those that failed to stand the accelerated tests, the lime was low, the sulphur low, and the time of setting quick, with the exception of the cases above referred to, where other elements entered into the calculation.

It is further to be seen that the only cement that contained a large percentage of lime, and that did not contain a large percentage of sulphur, but which was made slow setting by the large percentage of lime, failed to stand the ordinary normal cold-water test, thus corroborating Dyckerhoff's, Lewis's and De Smedt's views.

In view of this conclusion, it would seem from a careful examination of the history of cement testing and cement manufacture, that this method of treating cements with sulphate of lime came about substantially in this way. For many years manufacturers of Portland cement went on making cements of fair tensile strains at seven days neat and of fair average fineness. Later on, engineers began to demand higher tests neat at seven days and finer grinding, and the requirements of the old specifications of from 250 to 300 lbs. were run up to 400 and 500 lbs., and finer grinding insisted upon. This was all

done without reference to the fact that fine grinding tends to make the cement quick setting and low in tensile strain at seven days neat, though increasing its tensile strain in 2 to 1, and 3 to 1, sand mortars. The manufacturers, then, in order to meet these new requirements, soon ascertained that it was necessary to increase the percentage of lime in their cements in order to make these very fine ground cements slow setting enough to give high tensile strains neat at seven days, according to the requirements of most engineers outside of Germany. The increasing quantity of lime went on until it ran into the excess of lime, which the cold-water test began to discover. This led to innumerable discussions on free lime and over-liming, to detect which the various accelerated tests above described were brought out, in addition to the well-established normal test which had already discovered these incorrect methods of manufacture. To meet these accelerated tests and also the still higher requirements for neat tests at seven days, manufacturers began to test various modes of adulteration. Ground iron slag was the first, but it was soon exposed. It was followed by sulphate of lime, which is now largely ground with many Portland cements for the purpose of producing the results sought after, that is, to make the cement slower setting and higher testing in neat tests at short periods. This adulterant has only been in use for several years abroad and in this country. It is true that some manufacturers in America have used it for as long a period as a year or 18 months, but there are, so far as I can find in the books, no records of works done with cements in which this material has been used as an adulterant that have yet stood the long periods of time that the Portland cements made in the ordinary and approved way have stood, while it is well known that there have been a number of questions raised and referred to in the books, where the destruction of certain mortars used in salt water have been attributed to the sulphate of lime produced by the decomposition of the sulphate of magnesia in cements; as to this, Candlot, on page 253, states that in all cases where cement is kept in salt water, and the proportion of sulphate of lime exceeds 1 to 2%, the mortar never fails to show traces of alteration, and the briquettes are frequently completely disintegrated. On this point he refers to Mr. Feret's paper in the *Annales des Ponts et Chaussées*, March, 1890, page 67.

With this evident danger staring us in the face, the question is, is it wise for the manufacturer, in order to meet the innumerable accelerated tests that are proposed, to adulterate his cement with a material which leading authorities by repeated experiments have shown will, if used in any large amount, render it useless in salt-water construction? This question especially presents itself to the American manufacturers of Portland cement doing business with the coastwise cities of the United States, as well as to the foreign manufacturers supplying

the maritime countries of the world. It would be simply a reckless risk for a manufacturer to take, to use sulphate of lime in percentages over 1% as an adulterant of his materials, and especially for any manufacturer to put such cement on the market, unless he had assurance from each and every party purchasing his material, that it would not be used in salt-water construction.

Stood.				Failed.			
No.	CaO.	SO_3.	Time of Setting. Minutes.	No.	CaO.	SO_3.	Time of Setting. Minutes.
I........	64.80	0.22	210	IV.........	62.80	0.49	8
II........	64.48	1.03	210	V.........	62.88	0.49	4
III........	63.60	1.64	360	IX.........	64.30	0.49	6
VI........	64.08	1.47	195	X.........	61.20	1.31	70
VII........	63.60	1.17	540	XII.........	62.96	1.10	300
VIII........	63.88	1.19	480	XIII.........	65.20	0.67	120
XI........	61.10	0.63	15	XIV.........	63.60	0.63	35
XV........	60.96	1.24	840	XVI.........	60.56	0.42	840
XIX........	64.56	......	540	XVII.........	59.60	......	60
XX........	......	......	600	XVIII.........	59.52	......	10
XXI........	50.56	1.28	345	XXIV.........	61.18	0.42	750
XXII........	59.74	0.88	660				
XXIII........	64.18	0.56	650				
	63.52	1.01	421		61.93	0.70	148

Thomas D. Whitaker, Esq.—I think this question of hot-water tests has considerable value, at least to the manufacturer. Mr. Faija has given this subject more time and study than anyone else, and still approves of a hot-water test, though somewhat moderate compared with the one in Mr. Lewis's paper.

We have used this hot-water test for a long time at our works, and I consider it a good test for the manufacturer. I do not, however, advocate it for the practicing engineer, for I think in many cases it may condemn a good cement; but the manufacturer, who knows his own cement and how it stands the hot-water test when in its normal condition, can easily distinguish any change that may take place in it by noting its behavior when subjected to this test, for its great advantage is its very marked results.

I think Mr. Lewis, in his paper, uses some terms improperly, for example, "free lime"; he does not designate any difference between free and hydrated or carbonate of lime. I take free lime always to mean caustic lime, and I believe this is the correct meaning of the word. I cannot see how we can call carbonate of lime in cement free lime, for it is combined with carbonic acid and is not detrimental beyond the amount of impurity it adds to the cement, whereas the caustic lime will swell by the addition of water and cause the cement to crack, and, if in suffi-

cient quantity, will cause complete disintegration. It is well known that the longer or harder lime is burned, the greater time it will take it to slake, so in a well-burnt Portland cement, any free lime that is present will be very hard burnt, and consequently will be very slow to slake, so after the cement has set or taken its form and commenced to shed the excess of water, that water will be taken up by the free or caustic lime, and when it begins to slake it will swell and break apart the bond in the cement and cause cracking and disintegration. It is also well known that hot water will cause this slaking of caustic lime to take place much more rapidly and this is the theory upon which this test is based.

In the matter of the addition of sulphate of lime to Portland cement I have experimented largely, and have found the addition of sulphate of lime in small quantities to be beneficial. These tests and experiments were carried on for more than a year, and the cement has improved in test up to that time, and in no case has there been any indication of disintegration or decrease in tensile strength, except when the experiments were carried on with sea water. In that case I found that a very small quantity of sulphate of lime should be used, as a small percentage would cause disintegration on account of the chemical action that takes place between the sulphuric acid and the alkalies of the sea water, and I do not think the limit of 2% of sulphuric acid in Portland cement too severe, for I believe an excess beyond this amount to be detrimental, especially if used in sea water. In ordinary tests, however, I have tried double this amount with apparently no detrimental effect.

Did I not understand Mr. Lesley to say that the cement that contained the greatest amount of free lime passed the hot-water test?

Mr. LESLEY.—No, it was detected in the cold-water test; I did not say free lime, but the highest in lime, and it contained 66 per cent.

Mr. WHITAKER.—I thought you said free lime. I was going to ask you how they determined it, for I don't think anyone here knows how, and it certainly would be of interest to us to know. You spoke of these cements being high in sulphuric acid by the addition of gypsum.

Mr. LESLEY.—I say they were made slow setting by the addition of gypsum.

Mr. WHITAKER.—Could you give us those percentages?

Mr. LESLEY.—Certainly, sir; you can see them in the table referred to.

Mr. WHITAKER.—They were all German cements, were they not?

Mr. LESLEY.—They were 24 German cements, so far as I am informed.

Mr. WHITAKER.—In English cements I do not think you will find any with less than 1 or 2% of sulphuric acid. The American cements will average about 1¼ to 1¾% of sulphuric acid.

Near the close of Mr. Lewis's paper he says that the disintegration of the cement in hot water is undoubtedly caused by the anhydrous sulphate of lime, that is, the sulphate of lime that is burned with the cement. I have experimented on that, and I must say I differ with Mr. Lewis. I have had cement that only showed $\frac{3}{10}$ of 1% of anhydrous sulphate of lime entirely fail in hot water, while others containing a much larger amount stand it perfectly. I have tried different percentages of lime, silicon and alumina in the composition of the cement, and found that as soon as the lime was somewhat low and the alumina high that the cement did not stand the hot water properly. My theory for this is that alumina is said to give an acid reaction when heated to a high temperature, for we burn our cement at a very high temperature (we use oil fuel in our kilns and get an exceedingly intense heat). The excess of alumina will need more lime for its combination, and there will not be sufficient lime remaining to combine, as tri-calcium silicate, with the silicon. The excess of silicon, having a greater affinity for the alumina than the lime, will rob the lime of its alumina, fluxing with it and leaving the lime in a free state. This is theory on my part, but I cannot account for this condition in any other way. And so I am convinced from my experiments so far, that the hot-water test does act on the free lime. I do not think it is on the aluminates of lime, and I again state that I think the hot-water test a good one for the manufacturer, and after 18 months' use at our works we still propose to continue the use of it, and can tell the least variation of our compound by its behavior in hot water.

Mr. Lesley says: "The cements which stood all the tests were very slow setting." Since this remark I have made some tests of exceedingly slow-setting cement without the sulphate of lime. It did not stand the hot-water test, and the addition of sulphate made it rather quicker setting, and it gave no better results in the hot water, although it was an exceedingly good cement, taking from four to eight hours for initial set, and showing about 700 lbs. tensile strength per square inch for a test of neat cement in a week, and 200 lbs. tensile strength per square inch, 3 parts of standard sand to 1 of cement in the same time. These are certainly good results, but this cement undoubtedly contained free lime, although not in sufficient quantity to be detrimental, but too much to stand hot water. I am still experimenting with this cement, and believe that it will lead to some very interesting results.

Robert A. Cummings, Assoc. M. Am. Soc. C. E. (by letter).—To the numerous essays on the subject of cement, concrete and kindred subjects, is added this paper on "Hot-Bath Tests." It is in a direction of research which is very inviting, and, judging from the abstract of the paper, very encouraging. It would be unwise to travel the ground of published comment, but the views of the authors may be elicited on a few questionable matters.

"There is at present only one way of determining whether the judgment passed on a cement by any system of testing is sound, and that consists in waiting half a century to see how the work stands," says Mr. M. H. Le Chatelier, in discussing the hot-bath tests.*

It is of paramount importance, therefore, to develop any method by which practical cement testing can be more rapidly carried out. This is especially the case with the hot-bath tests method.

There is an obvious advantage in having the chemical analysis of cement determined, and to the practicing engineer the immediate knowledge of the strength and quality of the cement on the work is of primary importance. It is well known there may be a variation of 25% or more in the tensile test method by different experimenters which is sufficient to warrant its condemnation.

The writer's experience has satisfied him of the necessity for careful manipulation of cements in testing by experts alone, whose qualifications command the confidence of engineers and manufacturers alike.

In hot-bath tests the precise and thorough mixing of the ingredients has been found necessary, to produce compactness and freedom from air bubbles which materially affect the result. Cement mortar mixed as dry as possible will occupy a much greater volume than the same quantity mixed with the proper proportion of water. The driest cement mortar will therefore be less compact.

The writer would endorse the views of the authors as to free lime, with the precautionary exception that improperly burned cements must not be overlooked. The unfortunate existence of free lime in certain work to the writer's knowledge is sufficient evidence of its importance and the necessity for its detection.

In a properly burned cement, made with the ingredients thoroughly mixed, there is no free lime. It may be advanced that the expansion of cements is due to the fine particles setting first and the coarse grains taking longer, or being incompletely hydrated. This may be the case with certain cements, and it would substantiate the necessity for uniform fineness in quality. The specific gravity of new cement will determine whether or not it has been insufficiently burned, and should therefore be embodied in all tests.

Why were not the hot-bath tests made at boiling point, which is easier to maintain than 180° ?

Did the authors use distilled water in their tests? If not, the soluble constituents of the local supply might account for some results.

The microscopical examination of the cohesiveness and conduct of cements opens a field upon which the authors may have some information.

* *Bulletin de la Société d'Encouragement pour l'Industrie Nationale*, 1890, p. 560.

Now that hot-bath tests indicate the quality of cement in a more or less degree, one is led to inquire what will low temperature or alternate freezing and thawing tests indicate?

It has long since appeared to the writer a somewhat one-sided conduct to have our metallic materials precisely specified and inspected by experts at the manufacturer's, while cements are frequently received on the work, without having any idea where they come from, their quality or age. The mechanical function of testing so many thousand briquettes in tension is performed by a promoted rodman, and "All's well that ends well!" so far as he knows.

It is needless to say there is a much-needed reform for the consistent management of this department, and the authors have done good work in bringing the matter forward so ably.

M. J. Butler, M. Am. Soc. C. E. (by letter).—Shortly after Mr. Faija's paper appeared the writer began to make use of the hot-bath test, and from that time on has always relied on it as being the surest and safest way to determine the soundness of a cement. It is believed by the writer that with a set of sieves 2 500, 5 476, 10 000 and 22 500 meshes to the square inch, for the purpose of ascertaining the fineness, a specific gravity determination apparatus and the hot bath, one can arrive at a safer conclusion as to the soundness and suitability of a Portland cement than by any other known means.

There can be no doubt that the hot test is being abused; excessive temperature will probably blow any coarsely ground chemically perfect cement; hence, in the absence of a test for fineness it may be misleading to that extent. Manufacturers of cement, knowing their cements will be subjected to the boiling test, are in self-defence compelled to keep down the percentage of lime with a consequent reduction in tensile strength; but any properly burned, properly ground Portland cement, containing 60% of lime, will stand the hot test, as laid down by Faija, although if fresh from the stone will probably blow if boiled. The authors seem to have discovered a useful part for sulphate of lime as a factor in cements.

William H. Booth, M. Am. Soc. C. E. (by letter).—I wish to say that I am in agreement with the authors as to the terrors of free lime in cement. Suitable air slaking will purge a cement of caustic lime and render it safe. Free or caustic lime may be rendered safer by fine grinding, which reduces the particles to flour and so evenly distributes them throughout the bulk of the cement as to render them easily hydrated, even if not perfectly air slaked. So far, however, as English cement is concerned, the whole of the manufacture is carried out upon low-lying, often marshy, land, near tidal rivers, in a humid atmosphere, and the process of air slaking is fairly rapid.

Until quite recently it has been the habit not to grind so finely in England as in Germany, perhaps in part because of the greater safety

arising from a humid atmosphere, and partly because of the superiority of the English raw material; but owing to German competition, the English makers are grinding finer, and I have no difficulty in getting cement ground to pass through a No. 80^2 sieve. I am aware of the addition of sulphate of lime to promote slow setting. This addition is made by the German manufacturers to meet American specifications, but it is not looked on here as an honest cement, and, if above a small percentage, will make a cement unsound, and it ought to be rigidly excluded. A pure cement ground from a well-burned clinker of a proper mixture of the simple raw materials, chalk and clay, will be a sound cement, for there is no impurity in the above two substances which will render a cement unsound. If ground from clinker that has not been picked over to remove portions insufficiently burned, there is, of course, introduced a foxy colored, quick-setting element which is, perhaps, unsafe, but such can only arise from simple neglect. The half-burned clinker only costs the wages of picking out, for it can be further burned in another kiln. I am, therefore, in very full agreement with the authors that sulphate of lime is a mischievous and improper adulterant. Indeed, anything added to a cement makes it worse, and engineers would do well to guard themselves against drawing such specifications as lead to adulteration, for it is to the impossible clauses in specifications that the sulphate mixture is due. What is known as the "first" set of a Portland cement is undoubtedly due to the presence of more lightly burned clinker. That a little may creep in as dust is, of course, unavoidable, and, so far, is of no serious harm, the slight initial set being an advantage; but there should never be so much present as to destroy the blue color of the cement.

L. C. Sabin, Assoc. M. Am. Soc. C. E. (by letter).—Several points touched upon by Messrs. Lewis and Whitfield are of much interest to users of cement. The following conclusions seem to be presented:

First.—That when free lime occurs in a cement it ceases to be caustic in a week or ten days.

Second.—That "thoroughly hydrated lime is of no disadvantage to a cement," but rather an advantage.

Third.—That if a defective cement is rendered slow setting by the addition of sulphate of lime, the hot test will not then detect its defects, even though these defects be accentuated by the addition of free caustic lime to the already unsound product; and hence—

Fourth.—The high-temperature bath is not of much value in detecting free lime.

The first conclusion seems to be based on the fact that when ordinary lime is ground to a powder it quickly hydrates, or slakes, in the air. The length of time required for lime to slake depends on its

purity, and the degree of heat to which the more or less impure raw material has been subjected in burning. It seems, then, quite conceivable that the "free lime" in cement may not slake in the air, or even in water, in anything like such a short length of time. This idea is not new.

That hydrated lime is of advantage to a Portland cement is an idea that is not only new, but one which, it is believed, will have to be supported by very substantial evidence before it is generally accepted. Mixing lime paste with lean Portland cement mortars is often recommended for reasons purely mechanical, but it is generally considered that replacing a portion of the cement with lime results in a proportionate decrease in strength. While it may not be actively injurious, it is not clear how hydrated lime can be considered anything but an adulterant as far as strength is concerned.

The third point mentioned above is one which, if proved to be true, is of great interest and importance. If a cement which is defective on account of free lime may be made to withstand the hot-bath test by the addition of sulphate of lime, it will detract very much from the usefulness which the hot test is supposed by some to possess. The first tests cited in the paper to prove this point show that after being rendered slow setting, the cement under consideration would not bear the addition of 10% of quicklime even when the cement mortar was left in the air. There would scarcely be a necessity then of resorting to the hot test. The briquettes apparently expended all their expansive energy in the molds, and when subsequently placed in the hot bath they developed some strength. This is analogous to the action of frost on mortars; the freezing of the water in the mortar may cause swelling and cracking, but with the approach of warm weather considerable strength may be developed. Concerning the tests numbered 3 085, etc., and 150, etc., the cement withstood the hot test after the addition of 3% of a hard-burned quicklime. By referring to Table III of Mr. Maclay's paper,* it will be seen that the hot test did not detect a 3% addition of lime if the pat was not immersed within less than 24 hours after being made. It was not surprising, then, that these briquettes withstood the treatment to which they were subjected.

As regards the remaining briquette tests it may be noticed in passing that the seven-day cold-water test did not give any indication of the very large amount of lime present. If this lime were of the kind mentioned in the early part of the paper as requiring but seven to ten days to slake, it is not strange that the cold test failed to show it, since the lime had remained in a box for a week; neither should the hot test be expected to detect it, as I have not seen it claimed by any writers on the subject that the hot test would detect hydrated lime.

* *Transactions* Am. Soc. C. E., Vol. XXXVII, page 418.

But a possible explanation of the failure of the cold test and partial failure of the hot test to detect the large amount of lime may be found in the nature of the latter as regards impurities and degree of burning. If the writers were able to show by subsequent results that this lime was such as to render the cement a dangerous one to use, or that the lime used was in the same state as that which is found in cements, then it might be said that the hot test should have brought out the defects of the cement more clearly. The "remarkable capacity of slow-setting cement to carry free lime without injury" does not, therefore, seem to be very clearly established, if "free lime" is taken in its ordinary meaning in this connection.

Neither is it quite plain what "question" is begged by saying that the free lime, which does no harm, need not concern us. What is usually claimed for hot tests is that they will detect tendencies to change in volume, and it is generally believed that free lime (not hydrated) is one of the most common causes of swelling. It is clearly stated by most writers that free lime is not the only cause of swelling.

In testing Portland cements, I use a temperature of 80° Cent. for hot-bath tests. This is practically the same as that used by Messrs. Lewis and Whitfield. This may not be the best temperature. It is possible that it might well be increased to a point somewhat below 100° Cent., and it may be that the temperature used by Mr. Faija is sufficiently severe.

The following results were obtained with a sample of cement which failed in the cold-pat test. A thin pat on glass, placed in cold water (15 to 18° Cent.) when it had set, cracked slightly at the edges in about two weeks, and a similar pat treated the same way, except that it was left two hours longer in the air, cracked in about four weeks. Two pats which were left in the vapor three hours and then immersed in the hot bath (80° Cent.) showed many fine cracks on the surface, had warped and one of them had nearly disintegrated in less than four days; these two pats were placed in the vapor when they would hold the ¼-pound wire and 1-pound wire, respectively (General Gillmore's wires). Some of this cement was then spread about 3 ins. deep, and placed in a dry place to aërate. At the end of one, two and three weeks the tests were repeated on the aërated sample with little, if any, better results. At the end of nine weeks' exposure, a treatment to the hot bath for two days showed that pats would not yet withstand the hot test, but in cold water one of the pats was still sound after two months, while the other was only slightly warped. After 15 weeks' aëration, the pats merely became detached from the glass after being in the hot bath two days. The following results were obtained with briquettes from this sample of cement, 2 parts sand to 1 cement, by weight. The hot-water briquettes were in moist air 21 hours; vapor, three hours;

and hot water (80° Cent.), four days. The cold-water briquettes were in air 24 hours before immersion :

Time aërated, days.	Temperature of water of immersion.	Age, days.	Mean tensile strength in pounds per square inch.	Swelling of 1-in. section.
00	80° Cent.	5	8	$\frac{1}{8}$ in.
21	"	5	18	$\frac{1}{16}$ "
63	"	5	186	
00	15 to 18° Cent.	7	258	
00	" "	28	368	

It may be held by some that the checking of neat cement pats, even in cold water, does not prove that the cement may not give good results when mixed with sand, and consequently that I have not shown that the cement under consideration is a bad one. However, I submit that it would not generally be thought that the manufacturer had been treated unfairly if such a product were rejected. I do not believe an engineer would get his full allowance of sleep if he thought his structure rested on that cement, yet a tensile test five months, neat, cold, showed a strength of about 540 lbs. per square inch, with no cracks visible in the briquettes. Had this cement been stored long enough to have aërated about the same amount as it did in nine weeks, when spread out, then before the two months required by the cold test to detect a tendency to blow had expired, the cement would doubtless have been in the work. As the hot-bath test showed this tendency in two days, I leave it as an open question whether it gave any useful information.

As is intimated above, there are but few who would not pronounce this sample unsound. As aëration improved its quality, the unsoundness might have been connected with the presence of free lime ; if so, the latter would not appear to be such a harmless element, and this free lime required more than 10 days to slake.

It has seemed to me that the promoters of the hot test have made greater claims for it than were warranted by the results which they have presented, and it is certain that inconsistencies exist which furnish points of attack for its detractors. Mr. Henry Faija has been satisfied with the results of his apparatus with a temperature of about 115° Fahr., after many years' use, but there are probably many engineers who have used the cold test alone, and have not met with any failures in their work which they attribute to unsound cement. In his paper on the subject Mr. Faija says :* "Some cements will blow whether they are new or old"; and again (page 59): "That nearly any cement that had aged sufficiently would stand the hot test"

* *Transactions*, Am. Soc. C. E., Vol. XXX, page 56.

(referring to 80° Cent.). Now, it seems certain that a cement which stands the hot test will also stand Mr. Faija's test, and hence his test would fail to detect tendencies to blow in those cements which were old.

The value of the hot test should be demonstrated by an extended series of experiments. It is not sufficient to compare the results obtained in the hot bath with 7 and 28-day results obtained in cold water. Before the hot test will be generally accepted, it must be shown that a cement which withstands this severe treatment will not prove defective in ordinary use after months, at least; and in justice to the manufacturer, it should be shown that there is not a large class of cements which will give the best results in practice but which will fail in the hot tests. But "Rome was not built in a day." Meantime, that the hot test is gaining in favor is evidenced by its extending use, and such papers as the one under discussion are very welcome, as elucidating the subject.

W. W. Maclay, M. Am. Soc. C. E. (by letter).—I have carefully read the paper by Messrs. Lewis and Whitfield without finding any reason to change my views about "Hot Tests for Portland Cement," as set forth in my paper*; on the contrary, I am to-day more strongly in favor of the "hot tests to quickly show the tendency of Portland cement to change its volume" than I was two years ago. My reason for feeling more positive than formerly in regard to the usefulness of the hot tests is because I have employed this test almost constantly since my paper was read, and always with satisfactory results. By this I mean that Portland cements that failed by exposure to the air for long periods, from three months to one year, by losing their early tensile strength or by disintegrating entirely, were invariably rejected in 24 hours by the hot test, and invariably passed all the cold-water tests. Great weight is always given in the discussions against the "Hot Tests for Portland Cement," to the fact that the German Normal Rules for testing do not recognize them. This fact would almost seem to settle the question against the hot tests, inasmuch as Germany is acknowledged to be the foremost country in the manufacture and testing of Portland cement. The opinion in Germany, however, against the hot tests is not nearly so unanimous as is commonly represented; some of their most eminent cement engineers are not only in favor of the hot tests, but are using them at the present time in addition to the Normal Rules, and, as for the manufacturers, I think there are few that do not now use the hot tests for their own information, whatever may be the practice with the consumers.

There will be a conference or congress of eminent cement engineers of different nationalities at Zurich, Switzerland, in September, 1895, where the whole question involving the change of volume of Portland

* Vol. XXVII, *Transactions*, Am. Soc. C. E., October, 1892.

cement will again be thoroughly discussed, and recommendations made; and, inasmuch as the German Normal Rules have been largely the result of these congresses, we need not consider the question as definitely settled against the hot tests quite yet.

The following is an extract from a paper sent to me by Dr. Michaelis, of Berlin, early in the present year :

"The so-called American hot-water test, described in No. 12 of the *Thonindustrie Zeitung*, of March 18th, 1893, has been, as it is known, but with slight alterations, used by me for many years. I always recommend it for the purpose of selecting the most reliable cement in the shortest time. Therefore I welcome the impartial experiments on this method by the American engineer, W. W. Maclay, and his colleagues in the field."

"For ten years I have endeavored, but without success, to prove the value of an accelerated testing of Portland cement to the Ministry of Public Works in Prussia. Alas, it is unfortunate to be so far ahead of the age. At the present time the chances look somewhat better."

The writers of the paper under discussion are inclined to think the question of free lime a "bugaboo," and argue that because hard-burned lime ground to pass a No. 74 sieve soon ceases to be caustic, and rapidly takes up water from the air; therefore, it is probable "that in a week or 10 days after grinding, the free lime contained in a cement has ceased to be caustic." This theory may seem plausible to the writers, but it is not very satisfactory to the manufacturer who finds himself obliged to cure his cement six months before he dares to put it on the market.

Some space in the paper is devoted to proving that hydrated lime is of no disadvantage to a cement. This, of course, admits of no discussion, for as soon as the free lime in a cement has been hydrated, it will pass all hot tests.

The addition of small quantities of sulphate of lime to properly burned and carefully proportioned artificial Portland cement to make it slow setting has long been practiced in the German factories, and doubtless enables some of their cement to pass the hot tests, which they would not do before the sulphate of lime was added. But to follow this practice with a really objectionable Portland cement, containing a large excess of free lime, would require the addition of sulphate of lime beyond the recognized limit.

The effect of adding caustic lime to Portland cements, given in Table I of my paper, already referred to, does not agree exactly with the results recorded in the paper under discussion, but this may be due to the fact that some of the lime used in these experiments "had been ground up fine and kept in a box in the air for a week," when, of course, it would cease to be caustic.

Mr. Rudolph Dyckerhoff, in discussing my paper in 1892, is quoted in this paper as saying:

"When thoroughly hardened, the mortar of any good Portland

cement will safely pass this violent test," and the writers of the paper go on to say from this, "It is difficult to escape this conclusion—if a cement will not stand boiling in a fresh pat because of free lime, but will stand boiling in a pat a few days old, then it must in the interval have disposed of its free lime."

Mr. Dyckerhoff's conclusion quoted above, and the writer's deduction therefrom, are both erroneous, as well as the remarks, in this connection, made by Mr. Dyckerhoff at the general meeting of cement manufacturers in Germany in March, 1893, viz.: "What, in fact, is meant by a cement not standing the boiling test? Nothing else, but that the cement mortar at a certain state of its hardening, after 24 hours, does not resist the boiling water; after a longer time, say, 14 days, the very same cement will endure the boiling test, etc." My own experience is exactly the reverse of Mr. Dyckerhoff's. Cements that would not stand the boiling test at 24 hours after gauging I have kept from three to four months immersed as neat briquettes, and even after that long interval of immersion in normal cold water these briquettes still failed to pass the boiling test. Pats and briquettes from these cements, kept in the air, cracked and disintegrated after two or three months' exposure, although the normal cold tests for short and long periods gave excellent results. It was, therefore, very gratifying to receive from Dr. Michaelis a paper completely confirming my own experience and refuting the views of Mr. Rudolph Dyckerhoff in this connection. The following is in Dr. Michaelis' own words: "In 1875 the undersigned (Dr. Michaelis) found with a South German Portland cement, renowned for its remarkable strength in common water hardening and not showing any increase of volume or decomposition in the air, that the briquettes which hardened and remained sound for months in the water were totally destroyed after but half an hour's boiling. Any cement that is not perfectly constant in volume and that shows an increase of volume 24 hours after the pats are made and boiled three or four hours will show the very same after 2, 3, 7 and 14 days of hardening (the first day in moist air, and after that under water), when they are afterwards submitted to the boiling test. Of course this can be changed by seasoning the cement from 7 to 14 days before making up the test pat and submitting it to the boiling. The boiling is not intended to prove whether the cement will be constant of volume in future time, but whether it is so when mixed and tested."

"Mr. Dyckerhoff will probably change his opinion after thoughtful study of the boiling test.

"On the other hand, it has been proved to be wrong, when Mr. Dyckerhoff says, 'Every Portland cement standing the standard test for constancy of volume will, in practical use, even at normal temperatures up to between 104 and 122° Fahr. (40 and 50° Cent.), prove always constant in volume for all future time.'"

I think, therefore, that while the writers of this paper are technically right in giving considerable weight to the German decision against hot tests, on the other hand, the friends of the accelerated tests need not despair of their views being almost universally accepted

at some future time, when they count as they do now, some of the ablest cement authorities in Europe as their advocates.

Messrs. Frederick H. Lewis and J. Edward Whitfield.—The following experiment is perhaps more convincing than anything which has yet been said, viz.: Take freshly ground and quick-setting cement and treat it with 2% of sulphate of lime, using preferably what is known as boiled plaster. The cement will at once become slow setting, and in nearly all cases it will at once stand the boiling test. Now take the sample thus treated and spread it in a shallow box, freely exposed to the air, so the free lime present can take up moisture and carbonic acid, and then test the cement every few days, to ascertain its setting time. It will be found that the time becomes shorter and shorter, until, at the end of two or three weeks, the cement is nearly as quick setting as it was before the plaster was added. When the cement has reached this condition, add to it 2 or 3% of quicklime, and again test the setting time. It will be found that the cement at once becomes quite slow setting again.

The explanation of these phenomena is simply this, that the cement can be slow setting only in the presence of quicklime. This may fairly be called *reductio ad absurdum* for the boiling test for free lime, because it is demonstrated that the sulphate has no effect whatever upon the lime, but that its reaction is with the aluminate of lime. As in many other chemical reactions, the presence of free lime is merely auxiliary. We owe these facts to Candlot, who developed them in an elegant series of experiments.

Now, no prejudice in regard to cements is more widespread than that the quick setting is due to the presence of lime. It is generally assumed that this is a self-evident truth, axiomatic. Yet Mr. Lesley points out that the addition of lime to cements has been practiced commercially, for the purpose of making it slow setting, and it is a very simple experiment to demonstrate that the addition of a small percentage of quicklime will generally retard the setting, and sometimes, as noted above, will produce a marked difference in the setting time.

It can hardly be questioned that the prejudice referred to accounts for the ready reception of the boiling test. When an experimenter finds that the quick-setting cements generally fail in this test, its value is at once established, because he knew (?) beforehand that the quick-setting cements contained free lime.

Mr. Whitaker objects to the use of the term free lime, and the writers in a measure agree with him. The word lime means, both in common and scientific use, calcic oxide (CaO), quicklime; or its hydrate ($CaOH_2O$), slaked lime, and nothing else. But just what is meant by "free lime," used in connection with the boiling test, we should prefer to leave for explanation to its advocates. It has always

seemed to us that Mr. Faija was more modest, and perhaps much more accurate also, in simply claiming for his apparatus that it would detect a "blowey" tendency in cements.

Covering the questions raised by Mr. Sabin on the propriety of using mixtures of Portland cement and slaked lime, we give below the records of tests by Candlot, referred to in the original paper.

First Series.

Composition of mortars for 1 cu. m. of normal sand, weighing 2 870 pounds.		Per cent. of water.	Tensile tests of briquettes, in pounds per square inch.				
			7 days.	28 days.	3 months.	1 year.	2 years.
1 215 lbs. of these different mixtures of lime and cement:	Cement, 80; Lime, 20 ..	11.0	223	300	424	537	512
	Cement, 65; Lime, 35 ..	12.5	207	285	363	514	455
	Cement, 45; Lime, 55 ..	13.0	145	205	322	469	425
	Cement, 30; Lime, 70 ..	13.0	121	185	286	368	375
Weights of a mixture of 70% cement and 30% lime for 1 cu. m. of sand (2 870 lbs.):	550 lbs.......	10.0	78	134	158	213	219
	990 "	10.0	201	269	322	478	482
	990 " (air)...	10.0	235	370	441	533	766
	1 440 "	10.0	334	357	488	585	636

Second Series.

Composition of the mortars.		Tensile tests of briquettes, in pounds per square inch.					
		7 days.	28 days.	3 months.	6 months.	1 year.	2 years.
Cement, 90; Lime, 10	1 part to 3 sand	229	310	377	397	415	451
Cement, 80; Lime, 20	" "	200	283	328	361	420	435
Cement, 70; Lime, 30	" "	139	193	252	297	318	371
Cement, 60; Lime, 40	" "	122	175	226	276	306	320
Cement, 100; Lime, 0	" "	142	229	292	368	377	437
	" " (air)..	132	229	296	446	577	711
	" 5 "	67	91	148	148	180	202
Cement, 70; Lime, 30	" 3 "	162	218	295	401	390	454
	" 3 " (air)..	202	255	361	352	484	484
	" 5 "	80	107	128	166	178	215
Cement, 60; Lime, 40	" 3 "	148	192	238	277	308	425
	" 3 " (air)..	169	277	385	344	458	727
	" 5 "	....	90	116	134	155	199
Cement, 50; Lime, 50	" 3 "	85	116	162	159	193	269
	" 3 " (air)..	108	201	267	222	378	660

It would appear from the way in which the mixtures are described in the first table that these tests are actually mortar box tests. It is, of

course, essential for good results that the lime shall be thoroughly slaked.

Speaking of slaked lime, is it not a fact that free and uncombined slaked lime is, and must always be, found in cements? The prime factor * in Portland cement is: SiO_2, 3CaO; and the reaction with water is SiO_2, 3 CaO + aq. = SiO_2, CaO, 2.5 H_2O + 2 CaO, H_2O; that is, rather more than 40% of the cementing principle is simply slaked lime.

Many people ascribe the strength of lime mortars entirely to the absorption of carbonate acid from the air, and overlook the strong bond which lime forms with silica, due to a coating of silicate of lime on each grain of sand.

The large fund of information which Mr. Lesley brings to bear upon the discussion, and the interesting report he gives of the meeting of the German Society, make his paper very valuable, and we are glad to note that the standard test is confirmed in Dr. Pruessing's experiments.

But we disagree sharply with Messrs. Lesley and Booth in considering the addition of sulphate to be essentially bad and undesirable. On the contrary, it seems to us that the use of sulphate within the limits of the German specifications make it an admirable cement for fresh water or air exposures. Cements treated with sulphate possess the peculiarity of setting slowly and hardening quickly. They gain strength rapidly. We have tested cements whose initial set exceeded one hour and a half, but which practically attained maximum tensile strength neat in seven days. And it will generally be found that the treated cements give tests at seven days from 30 to 50% higher than the untreated cements. This is a valuable property, and one well suited for work in rapid progress.

It is true that the addition of sulphate can be overdone, and it is also quite true that for salt water exposures the amount of sulphate present should be limited to quite moderate figures. But these facts are well understood in European practice, and are provided for in specifications, and if our engineers are using cements indiscriminately the fault is theirs and not the manufacturer's.

We have reason to think that in the best practice the total sulphate in treated cements is frequently no higher than in untreated cements. Thus the analyses of two samples of treated cements showed sulphuric acid as follows:

No. 1	0.697%
2	0.886%

Both of these samples are confidently believed to have contained

* See Le Chatelier: "Recherches Experimentales sur la Constitution des Mortiers Hydrauliques."

added sulphate, and both of them are fit for use in sea water. On the other hand, the following analyses represent cements which contained no added sulphate:

No. 3	1.085%	sulphuric	acid (SO_3).
4	1.123%	"	"
5	1.281%	"	"
6	1.161%	"	"
7	1.206%	"	".

Treated cements containing higher percentages than those given in Nos. 1 and 2 are as follows:

No. 8	1.164%	sulphuric	acid (SO_3).
9	1.323%	"	"
10	1.247%	"	"
11	1.340%	"	"
12	2.295%	"	"
13	1.340%	"	"
14	2.108%	"	"

It will thus be observed that the total sulphuric acid in several of the treated cements is about on a par with the percentage in the untreated cements. This would indicate superior kiln work, keeping the so-called "combined" sulphate at low figures.

In conclusion, then, so far as free lime is concerned, the writers are compelled to discard the boiling test. Whether the test has any significance at all is perhaps still open for argument and for investigation.